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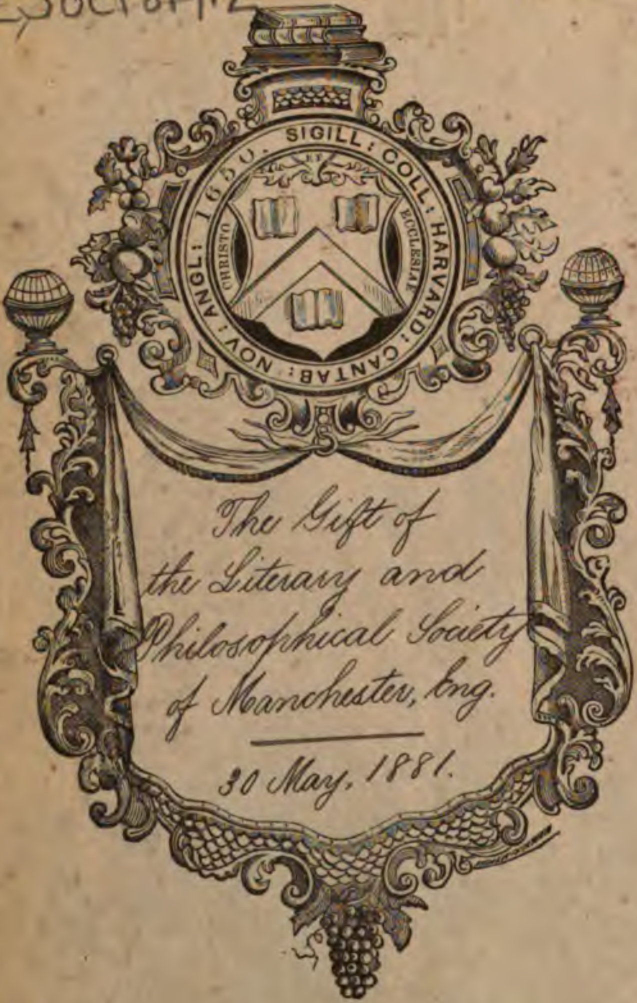
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NOTE.

THE object which the Society have in view in publishing their Proceedings is to give an immediate and succinct account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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ERRATA.

Page 34, for lines 8, 9, 10, 11, 12 and 13 read—"I can merely bring before you a few of the more noteworthy points which stand out from my general reading, more especially relating to the History of Ancient Life on the Earth, together with some general considerations which seem to me opportune under the circumstances of the time."

Page 40, line 4 from the bottom, for "fluviable" read "fluviatile."

Page 40, last line, for "the equivalent of" read "as old as."

Page 157, for "Cræstagus," line 4 from bottom, read "Cratægus."

Page 158, for "variation," line 5 from bottom, read "venation."

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 3rd, 1876.

Rev. WILLIAM GASKELL, M.A., in the Chair.

"On the Action of Water and Saline Solutions upon Lead," part II., by M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

1. I have already in several places* described the results of various series of experiments, undertaken with a view to arrive at definite measurements of the solvent action of dilute saline solution upon lead, and to ascertain the circumstances which condition this action. The general results of these experiments may be summed up briefly as follows.

(1) *Nitrates* cause water to exert a very marked solvent action upon lead.

(2) *Carbonates, Sulphates, and Chlorides* greatly diminish the solvent action of water upon lead.

(3) These three salts, *along with Nitrates,* greatly decrease or even stop the solvent action exercised by the Nitrates when present alone.

(4) The amount of lead increases with the length of time during which the water remains in contact with the lead.

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* Proc. Glasgow Phil. Soc. 1871-72, 184; Soc. Sci. Congress, 1874; Manchester Lit. and Phil. Soc., 1875-6, 35.

D.—EXPERIMENTS CARRIED OUT IN CORKED FLASKS NEARLY FILLED WITH LIQUID (500cbc.) THROUGH WHICH A STREAM OF AIR WAS CONTINUALLY PASSED.

Nature of Solution.	Grams per Litre.	Surfaces of Lead exposed in sq.cm.	Total Lead in Milligrams in Solution in the Liquid after								
			42 hours	68 hours	70 hours	138 hours	140 hours	168 hours	336 hours	340 hours	505 hours
Distilled Water	....	25	..	..	..	..	..	..	..	1·5	2·0
Potassium Nitrate	0·132	25	..	..	..	..	..	..	..	1·2	1·5
,, ,, ...	,,	240	..	0·8	..	..	..	..	..	..	..
Calcium Chloride......	0·20	25	..	..	..	..	..	3·5	8·5	..	8·5
,, ,, ...	,,	50	..	..	..	..	..	0·5	0·5	..	0·5?
Potassium Carbonate..	0·20	25	..	..	..	..	..	0·4	0·6	..	0·6
,, ,, ..	,,	50	..	..	..	..	..	0·5	0·5	..	0·5
Ammonium Sulphate..	0·20	25	..	..	..	..	..	1·3	3·0	..	5·0
,, ,, ..	,,	50	..	..	..	..	..	0·7	1·3	..	2·5

E.—EXPERIMENTS CARRIED OUT IN BEAKERS HALF FILLED WITH LIQUID (500cbc.) AND COVERED WITH UNSIZED PAPER: DIAMETER OF MOUTH OF BEAKER=11·5cm. THE LEAD WAS SUSPENDED IN THE LIQUID SO THAT AN AMOUNT OF SURFACE EQUAL TO THAT IMMERSED WAS EXPOSED TO THE AIR.

Calcium Chloride	0·20	25	..	..	..	..	..	1·4	2·5	..	8·5
,, ,, ...	,,	50	..	..	..	..	..	2·1	3·5	..	4·0
Potassium Carbonate..	0·20	25	..	..	..	..	..	traces	0·2	..	0·3
,, ,, ..	,,	50	..	..	..	..	..	0·5	0·7	..	0·7
Ammonium Sulphate..	0·20	25	..	..	..	..	..	0·8	1·5	..	2·5
,, ,, ..	,,	50	..	..	..	..	..	1·0	2·0	..	8·5

These results do not point to a regular increase of lead dissolved associated with increase of surface exposed. In certain cases the amount of lead dissolved does increase most notably as the surface exposed is increased, but in other cases it diminishes. The other conditions affecting the solvent action appear to exercise a disturbing influence upon that one condition the action of which it was desired to trace. Thus in the case of distilled water, whether the surface of lead exposed measured 25 or 50 sq. cm. when the action was allowed to proceed for 42 hours, the amount of lead dissolved was the same, nor did carrying out the experiment in a corked flask, a beaker, or a basin loosely covered with porous paper, materially affect the result. But when the action had proceeded for 340 hours, the conclusions to be drawn are found to be very considerably affected by the nature of the vessel employed. In closed flasks the amount of lead dissolved slightly decreased with increase of surface exposed; in beakers there was a very marked increase in lead dis-
lved with increase of surface exposed, while in basins again
rease of surface was associated with decrease of solvent

action. By looking at the experiments carried out in corked flasks I think it would be altogether unwarrantable to say that an increase of surface exposed is generally associated with an increase in quantity of lead dissolved. From the experiments carried out in beakers half filled with liquid and covered with porous paper I think the conclusion may be deduced that there is *generally* an increase of solvent action with increase of exposed surface: this is especially evident in the case of those salts which increase the solvent action (nitrates, &c.) and after the lapse of considerable periods of time—300 to 500 hours. The results of the experiments carried out in basins do not permit me to draw any general conclusion on the subject now under consideration: there is sometimes an increase, at other times a decrease, in the amount of lead dissolved associated with a fixed increase in surface exposed. It would almost appear as if exposure of the liquid to large surfaces of air was less fitted to promote solvent action than exposure to smaller surfaces of air. And the experiments carried out in flasks through which a stream of air continually passed seem to countenance some such conclusion as this. In these experiments there was invariably a diminution in the quantity of lead dissolved associated with an increase in the surface exposed. It is only in the last set of experiments carried out in beakers half filled with water, and loosely covered, and having one half of the lead immersed in the liquid and the rest exposed to the air,—that an increase in lead dissolved is invariably associated with increase of surface exposed. So far as the first enquiry is concerned these experiments do not warrant the assumption of an invariable increase in the quantity of lead dissolved associated with an increase in the surface of lead exposed to the action of the solvent.

5. Does free admission of air to the surface, or passage of air through the body of the liquid influence the quantity of lead dissolved? The bearing of the experiments upon this question will be best seen by tabulating the results so as to bring together the quantities of lead dissolved by the same liquid acting on a fixed surface, but under varying conditions of exposure to air.

This is done in Table II.

Description of Vessel.	Surface of lead.	Total lead (in Mgms.) dissolved after hours 42	168	336	340	505	Vessel.	Surface	Lead dissolved. hours. 42	168	336	340	505
A.—Liquid employed, Distilled Water.							**B.—Liquid employed, Distilled Water.**						
Corked Flask....	25	0·7			1·2	1·5	Flask	50	0·7			0·9	1·0
Beaker half-filled with water 11·5 cm. diam. at mouth	,,	0·4			0·5	0·8	Beaker....	,,	0·4			1·2	1·8
Basin, 14·5 cm. diam..........	,,	0·6			2·0	4·2	Basin	,,	0·8			1·5	3·5
Flask with air passed through	,,				1·5	2·0			✓				
C.—Liquid employed, Potassium Nitrate, 0·20 gram. per litre.							**D.—Potassium Nitrate, 0·20 gram. per litre.**						
Flask............	25	0·7			1·2	1·6	Flask	50	1·0			1·2	1·5
Beaker	,,	0·4			0·4	0·5	Beaker....	,,	0·6			2·2	2·5
Basin............	,,	0·8			2·8	6·0	Basin	,,	1·1			2·8	3·5
E.—Calcium Chloride, 0·20 gram. per litre.							**F.—Calcium Chloride, 0·20 gram. per litre.**						
Flask............	25		1·3	1·8		3·0	Flask	50		2·0	2·5		2·5
Beaker	,,		1·3	1·5		2·8	Beaker....	,,		2·0	3·0		3·5
Basin............	,,		2·0	3·0		5·5	Basin	,,		2·5	3·5		4·5
Flask with air ..	,,		3·5	3·5		3·5	Flaskwith air........	,,		0·5	0·5		0·5 ?
Beaker—lead partly suspended in liquid partly in air	,,		1·4	2·5		3·5	Beaker—lead partly suspended in air	,,		2·1	3·5		4·0
G.—Potassium Carbonate, 0·20 gram. per litre.							**H.—Potassium Carbonate, 0·20 gram. per litre.**						
Flask............	25		traces	0·3		0·3	Flask......	50		traces	0·3		0·3
Beaker	,,		0·2	0·3		0·3	Beaker.....	,,		0·2	0·3		0·3
Basin............	,,		0·5	0·7		0·7	Basin......	,,		0·7	0·9		0·9
Flask with air ..	,,		0·4	0·6		0·6	Flaskwith air........	,,		0·5	0·5		0·5
Beaker—lead partly suspended in air........	,,		traces	0·2		0·3	Beaker—lead partly in air......	,,		0·5	0·7		0 7 ?
I.—Ammonium Sulphate, 0·20 gram. per litre.							**J.—Ammonium Sulphate, 0·20 gram. per litre.**						
Flask............	25		0·5	0·7		0·7	Flask......	50		0·7	0·7		0·7
Beaker	,,		0·7	1·0		1·3	Beaker	,,		0·7	1·0		1·5
Basin............	,,		2·5	9·0		16·0 ?	Basin......	,,		2·5	7 5		12·0 ?
Flask with air ..	,,		1·3	3·0		5·0	Flaskwith air........	,,		0·7	1·3		2·5
Beaker—lead partly in air	,,		0·8	1·5		2·5	Beaker—lead partly in air......	,,		1·0	2.0		3·5

I have not tabulated the whole of the results here, but only those which are directly comparable with one another.

6. It is scarcely admissible from these experiments to conclude that exposure to air invariably causes an increase in the quantity of lead dissolved. As in the consideration of the influence of surface exposed, it was found to be difficult if not impossible to eliminate other circumstances which modified the action, so here we appear to have many conditions tending to overshadow the effects of that one which it was especially desired to study. If we compare the quantities of lead dissolved in corked flasks and in open beakers, the action appears to be greater in the former than in the latter cases, until we come to deal with actions allowed to proceed during considerable periods of time, and upon somewhat extended surfaces of lead. When the surface exposed extended to 50 sq. cm. (to 500 cbc. of liquid)—and the time of action amounted to 300—500 hours, the exposure of the surface of liquid to a considerable surface of air invariably increased the quantity of lead dissolved in a given time.

In these experiments the surface of liquid exposed to the air was increased from about 2 to about 100 sq. cm.

By comparing the quantities of lead dissolved in flasks or beakers and in open basins we find that there is invariably a very marked increase in the latter cases. The increase here also becomes more marked when the action has been allowed to proceed for tolerably extended periods of time.

In these experiments the surface of liquid exposed to the action of air increased from about 2 to about 170 sq. cm.

It may be that the relation between lead exposed and total quantity of liquid influences the action of the air upon the metal: this point I propose to examine in a further communication. The passage of air through the various liquids certainly caused an increase in the quantity of lead dissolved as compared with those quantities found when the action was allowed to proceed in closed flasks; nevertheless, in every case—with one exception—considerably smaller quantities were dissolved when air was passed through the liquids, than when large surfaces of liquid were merely exposed to the action of the superincumbent air.

I have already pointed out, when considering the influence of the extent of surface of lead exposed, that the only experiments in which a constant increase in lead dissolved (independent of the salt in solution, the time, &c.) was noticed, were those in which the lead was partially suspended in the liquids and partially surrounded by air, the liquids being contained in beakers and exposing a surface of about 100 sq. cm. to the surrounding air. If we compare the quantities of lead dissolved under these conditions with the quantities dissolved in experiments carried out in a precisely similar manner, except that the lead was wholly surrounded by liquid, we find that there was a small but constant increase in the former cases; the quantities dissolved in these cases were not so large as those which passed into solution when the experiments were carried out in basins and the lead was wholly immersed in the liquid. On the whole then the exposure of the various liquids to a large surface of air appears to cause an increase in the quantity of lead dissolved: this increase becomes specially marked after the lapse of considerable periods of time.

7. Do the solvent actions of dilute saline solutions upon lead continue during lengthened periods of time, or is there a limit reached after which little or no further action is exercised upon the lead?

By consulting the two tables it becomes very evident that so far as these experiments allow one to judge there is a constant increase of lead dissolved with increase of time of action, except in the case of those solutions which contain carbonate of potassium. This increase appears to be proportionately greater in the case of those salts (nitrates, &c.) which aid the solvent action, than of those which tend to stop the solvent action of water upon lead. This increase is also greater for equal time-intervals, when a large surface of liquid is exposed to the surrounding air than when a small surface is so exposed. The increase was not very marked when the experiments were conducted in flasks through which a stream of air was constantly passed.

The exception which I have made in favour of potassium carbonate, when laying down the general rule that increase of duration of action increases the quantity of lead dissolved, requires explanation. On examining the actual numbers obtained it is evident that the amount of lead dissolved by liquids which contained potassium carbonate did increase as the action proceeded, up to a certain point: this increase was however very slight and after the expiry of 340 hours it ceased. Hence I conclude provisionally that in the presence of this salt the solvent action of water upon lead soon—comparatively speaking—reaches a maximum. I intend to investigate this subject more fully in a future communication.

8. In conclusion, it appears to be shown by these experiments that the solvent action of dilute saline solution upon lead tends to attain a maximum when large surfaces of liquid are exposed to the surrounding air, and when the volume of liquid is large in proportion to the surface of lead exposed. Further, that under these conditions, and in the presence of those salts which aid the action — especially nitrates and more especially ammonium nitrate—the quantity of lead dissolved increases in an increasing ratio with the time during which the action is allowed to proceed.

Many experiments must however be yet carried out before I can permit myself to generalise with safety, and these experiments must be conducted on a larger scale before the results obtained can be applied to the actual conditions which influence the mutual action of water and lead in our domestic water supply.

9. By comparing the absolute quantities of lead dissolved as stated in the foregoing tables with those tabulated in former papers and obtained under somewhat comparable circumstances, it is apparent that the present numbers are much smaller than the former. This I believe to be due to the chemical purity of the lead itself. In former experiments I made use of ordinary sheet lead: in the present experiments what is sold by the chemical dealer as "pure lead" was employed. I believe that many contradictory results noticed in the numbers obtained by different experimenters on the subject of the action of water on lead can be traced to slight differences in the purity of the lead employed by them. I purpose to examine this subject quantitatively, and hope on a future occasion to lay the results before the Society.

Ordinary Meeting, October 17th, 1876.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

Mr. BAXENDELL drew attention to a paper "On the protection of buildings from lightning," read by Prof. J. Clerk Maxwell at the late meeting of the British Association at Glasgow, and stated that the system of protection recommended by the Professor, and which he appears to have regarded as new, was suggested, and its adoption strongly advocated, nearly forty years ago by the late Mr. Sturgeon, whose many valuable contributions to electrical and magnetical science seem to have been strangely overlooked by recent investigators and writers. The paper in which the system was first described was read before the London Electrical Society on the 7th of March, 1838, and an abstract of it was published in the 2nd vol. of the "Annals of Electricity." There is however one important difference between the two systems. Mr. Sturgeon considered it necessary that the copper sheathing or covering of a protected room or powder magazine should be well connected with the ground; but Prof. Maxwell is reported to have stated that "there would be no need of any earth connection. They might even place a layer of asphalt between the copper floor and the ground, so as to insulate the building." It is obvious, however, that if the magazine were struck by lightning a disruptive discharge through the layer of asphalt would in all probability take place, which might rupture the copper sheathing, and thus ignite the contents of the magazine; but by the adoption of Mr. Sturgeon's plan an accident of this kind could not occur.

Remarkable Meteor.—The **President** said that on Tuesday the 15th of August last, whilst standing on the deck of the packet in Douglas Bay, in the Isle of Man, he observed one of the most brilliant meteors that he had ever seen. At about 35 minutes past 9 o'clock p.m., Greenwich time, he was looking towards the west, when he observed a body, apparently larger than the planet Jupiter, in the S.S.E., at an elevation of about 30° with the horizon, traversing the heavens at first in a nearly horizontal direction, then gradually declining and finally disappearing by a steep curve to the N.N.W. When first observed it appeared to be of a yellowish colour, but it changed to a greenish blue before it sank out of sight. During its last stage it reminded him of a large Roman candle, and from its great brilliancy appeared to be not very far from the place where he stood, but he observed no signs of its bursting, and it left little trace of a luminous track behind it. The speed at which it travelled appeared to be less than that of most meteors which he had observed.

This meteor appears to have been observed by several parties, for Mr. J. P. Norris wrote to the *Times* from Abbey House, Bristol, under date of August 15, "A brilliant meteor has this moment fallen due west of this house. It first appeared in the neighbourhood of Arcturus, then seemed to burst and trail light of rainbow colour, and was visible to near the horizon slanting towards the north. Its distance cannot have been great, for we saw it throughout two thirds of its course against a dark cloud. It may have fallen therefore in the neighbourhood of Clevedon." A correspondent, writing from Further Barton, Cirencester, on Wednesday, says, "At about 9.30 yesterday evening (15th) a magnificent meteor was seen from this place, passing slowly across the north-western heavens about midway between Arcturus and the horizon. The colour was vivid pale green, it left a greenish wake behind it, and burst with brilliant scintilations of whiter light"—and another says, "At 9.30 last night, Greenwich time, I saw the finest meteor or fire ball that it has ever been my fortune to observe. It passed just below E Bootes and travelled northwards in a descending direction between A Canes Venatici and the large cluster in Coma, rather nearer the latter. It exactly resembled the globe of fire projected by a Roman candle; the colour was of a brilliant yellow, and then after changing to a vivid green the meteor disappeared. The ball was pure and unattended by luminous track."

He gave the above particulars to show how observers were deceived as to the distance of meteors. The party who observed the one on the 15th of August near Bristol thought that it fell near Clevedon, while he (the President) seeing it at Douglas, 220 miles N.N.W. of that city, imagined it at no very great distance from him. He brought the matter before the Society for the purpose of enquiring whether the meteor had been observed by other parties, especially residents in Belfast or Glasgow, in order to ascertain if it had been seen westwards of those two places.

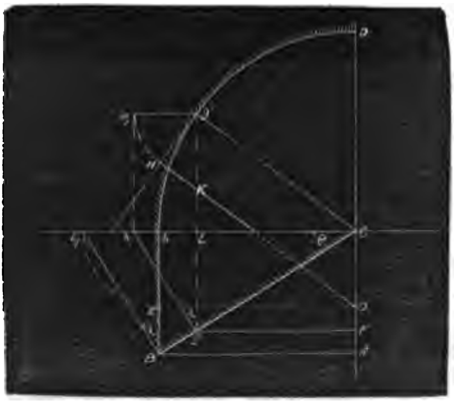
Mr. A. M. WORTHINGTON described the changes which take place in the forms of drops of liquids falling vertically on a horizontal surface, and exhibited the apparatus used in his experiments, and also a series of smoked glass plates bearing the impressions produced by the falling of drops of liquids from different heights.

"On the Directions of the Face Joints of Oblique Arches," by J. B. MILLAR, B.E., communicated by Professor O. REYNOLDS, M.A.

It was observed by Mr. Buck, as described in his well-known "Essay on Oblique Bridges," that in the drawings of the faces of oblique arches when the face joints were drawn straight lines and produced, they all met in one point on the vertical line intersecting the axis of the arch. That point he called the "focus" and its distance from the axis of the arch the "eccentricity." He gives no reason for this convergence of the face joints; but taking it as a fact, he determined the focus for the joints at the springing, and so obtained an expression from which the eccentricity may be found for any given arch.

In the Philosophical Magazine for January, 1862, G. B. Airy, Astronomer Royal, demonstrated, by the method of analysis, the truth of the theorem for the tangents to the face joints at points equally distant from the axis; such, for instance, as the points on the ellipse formed by the intersection of the soffit with the plane face. The following demonstration, besides being shorter than that given by the Astronomer Royal, depends only on elementary geometrical principles, and, it is believed, will convey a clearer notion of the state of the case, at least to engineers, whose point of view for subjects of this kind is frequently different from that of the mathematician.

The coursing joints of an oblique arch are *helical surfaces* of equal pitch; that is to say, they are surfaces generated by a straight line which revolves uniformly about the axis at right angles to it, and which at the same time has a uniform motion along the axis; the ratio of these two motions being constant for all the joints. A face joint is the line of intersection of one of these helical surfaces with the plane of the face. It will readily be seen that these face joints are curves, but the curvature is very slight for the ordinary dimensions and angles of arches. The only joint which could be straight would be a vertical one, and there is none such in the face of an arch.



Let BC represent the projection of half the face of the arch on a horizontal plane containing the axis, which passes through C, and bg'D the elevation of the soffit on a plane perpendicular to the axis; it is required to show that the tangents to the face joints at any points on BGD (G is the point on the soffit of which gg' are the projections) meet at one point on the vertical line CA.

The tangent to the face joint at any point is the line of intersection of the plane face and the tangent plane to the helical surface at that point.

First.—To find the tangent to the face joint at the point B. The tangent plane to the helical surface at B contains the generator AB and the tangent to the helix which passes through B, that helix being the line of intersection of the helical surface with the cylinder which is the soffit of the arch. These two lines determine the tangent plane. As the tangent to the helix at B lies in the tangent plane to the cylinder and makes an angle ϕ, which is the angle of skew-back of the soffit, with Bb, the point in which it intersects the vertical plane of projection may be found as follows:—

Draw BE$_{,}$ making an angle ϕ with Bb, and make bE equal bE$_{,}$; E is the vertical trace of the tangent to the helix at B —BE$_{,}$ is, in fact, the position of the tangent line in question when the tangent plane to the cylinder is turned about Bb till it becomes horizontal. But the tangent plane to the helical surface at B also contains the line AB, which is parallel to the vertical plane of projection; consequently EO, parallel to bC, is the vertical trace of the tangent plane. Hence it follows that the line of intersection of this plane with the plane face Bb passes through O (in elevation), and since it also passes through B, Ob is the vertical projection of the tangent to the face joint at B.

Second.—Let any other point G be taken of which the projections are g and g'. As before, the tangent plane to the helical surface is determined by the generator GF and the tangent line to the helix traced on the soffit through G.

The tangent to the helix lies in the plane Gg'H, so that by drawing $gh_,$, making the angle ϕ with gL and setting off g'H equal to L$h_,$, the vertical trace of the tangent to the helix in G is found. Hence the vertical trace of the tangent plane to the helical surface at G passes through H and is parallel to g'C, the vertical projection of the generator at G.

Draw HK parallel to Cg', meeting the vertical line g'L at K.

As the triangles KHg' and CLg' are similar,

$$\frac{Kg'}{Hg'}=\frac{Cg'}{CL}=\frac{Cb}{CL}=\frac{Bb}{gL}=\frac{bE_,}{Lh_,}$$

But Hg'=L$h_,$ by construction,

Therefore Kg'=bE$_,$=CO.

Consequently the line HK produced will pass through O and the vertical projection of the tangent to the face joint at G is Og'.

In the same way it may be shown that the tangent to a face joint at any other point of AGD passes through O. Q.E.D.

Let the angle BCb=ϑ=angle of obliquity of the arch.

Then Bb=$r \tan \vartheta$, where r = Cb, the radius of the soffit.

E$_,b$=B$b \tan \phi$ = CO.

Therefore CO = $r \tan \vartheta \tan \phi$.

This is the same value for the eccentricity as that found by Buck, and is perfectly general, depending, as it does, only on the radius of the cylinder, the angle of skewback for that cylinder, and the obliquity of the arch. The simplest method, however, of obtaining the point O is that shown above by means of a geometrical construction.

Ordinary Meeting, October 31st, 1876.

Rev. WILLIAM GASKELL, M.A., in the Chair.

Professor **REYNOLDS** said that he thought the manner in which some of the Manchester papers had heralded the return of the Arctic Expedition very unfortunate. *Failure of the Arctic Expedition. Collapse of the Arctic Expedition* appeared in large capitals. And although these might be explained as referring to the failure to reach the Pole, this was not the impression they conveyed to the public mind. Many readers of the newspapers never get beyond the large type, and of those who do, a very large proportion accept the decision of the paper as expressed in the heading without question. In this case the result appeared to have been to create a very general impression that the expedition had failed in its object; whereas, in truth, it had been one of the finest achievements ever accomplished. Looked at boldly it comes to this. Since Hudson's time, more than 200 years, Arctic Navigators had succeeded in penetrating about 60 or 70 miles of the 540 to be passed before the Pole could be reached. Whereas Captain Nares has, in one year, carried the British Flag some 60 miles nearer, so that nearly one half, and this by far the most difficult half, of the entire result of all expeditions since Hudson's time, has been accomplished by the last. And this is not all. Captain Nares seems to have pursued the journey to its end, at least by that route; and in coming back can say that he did not leave a single uncertainty behind him. So far, therefore, from having been a failure, this has been the most successful expedition ever sent out.

Mr. William E. A. Axon, F.S.S., exhibited a volume entitled "Gildas Salvianus ** By Richard Baxter. London, 1656." On the title-page was the autograph of Henry Newcome, M.A., the founder of Noncomformity in Manchester, and predecessor of the Rev. W. Gaskell, M.A., as minister of the original chapel in Cross-street. Newcome has carefully corrected the errata in the book, which he had evidently read with care. His diary contains the following characteristic notice of the book:—1692, "November 21st. I read, as I had time, in 'Gildas Salvianus,' which did greatly trouble me to see how sadly I fall short of what a minister of Christ should be."—*Autobiog. of Henry Newcome, M.A. Ed. by R. Parkinson, D.D. Cheetham Soc.* 1852. *p.* 276.

"On a Portrait of Tycho Brahe." By Samuel Crompton, M.D.

Dr. Crompton exhibited a portrait, in oil, of Tycho Brahe, and gave some particulars respecting the known portraits of Brahe. This picture is on canvas, and is 3 feet and 3¾ inches high, and 2 feet and 6½ inches wide. It represents a man of ruddy complexion, standing and looking forwards. He is bareheaded; has little hair and that short on the head of a yellowish colour verging to red. He has very long moustaches and a short beard. He wears a wide ruff round the neck edged with wide open lace. He wears a black jacket buttoned down to the waist with gold buttons, and he is decorated with the Danish Order of the Elephant and Castle, suspended by a gold chain from his neck, with a portrait in profile of the king in front of the body of the elephant, and below the elephant the letters MHZGA. He has a broad belt of gold round the waist. The right hand is nearly at a right angle with the body and rests on his side, and on its little finger there is a curious twisted ring of five turns. In his left hand he holds a pair of gloves; and on the left forefinger there is a large signet ring. There is also a ring on the fourth and another on the fifth finger. He has wristbands edged with open lace.

In the right upper corner of the picture (that is, to B.'s right,), there is a curious emblematic design, consisting of a round tapering column springing from a square base, around which at its foot are waves. Over the monument is a canopy suspended by a strong chain, a few links only of which are visible, the top being lost in clouds, and the chain itself has flames playing round it. Two Æolic heads (one on each side) are represented as blowing towards the canopy and column. Lower down, and to the right and left of the column, are two hands (one on each side) holding each a jug from which water flows. Clouds and lightning surround the background, the wrists of the hands holding the jugs, and also the Æolic heads. Round the monument is a label not entirely decipherable with the words: "Stans (tectus?) in solido;" then follows an indistinct word and "igne e · tunda" (*sic.*) "Igne et unda" was no doubt intended. In the left upper corner, in large and distinct letters, is this inscription: "Effigies Tychonis Brahe, Otton. Da. anno 50 completo quo post diutinum in patria exilium libertati desideratæ divino provisu restitutus est."

Dr. Crompton pointed out, by references to Gassendi's "Tychonis Brahe Vita, 4to, Hagœ Comit. 1656," that the inscription referred to Brahe's departure from Denmark, and that the "exilium *in* patria" was an allusion to his residence on his island of Huenna, in his Observatory, away from the court for twenty years. The emblematic picture evidently implies that nothing (not all the elements) could destroy the monument he had erected to his reputation by his observations; and that that they would be protected by Providence.

Dr. Crompton exhibited two other engraved portraits of Tycho: the first of them engraved by Meurs, in which he is surrounded by heraldic coats of arms, and with an inscription to the effect that the portrait represented him on the completion of his 40th year in 1586. The second engraving was by Haas, of Copenhagen, and is without a date. In both of these engravings Brahe is represented with a cap on his head and wearing, suspended by a double chain, the Danish Order of the Elephant and Castle, but without the portrait of the king, and without the letters on Dr. C.'s portrait.

Dr. C. proceeded to show from Gassendi, page 71, that King Frederick had given him the Order; and that subsequently his son, King Christian (Gassendi, page 116), had given him a second badge, which he himself wore;—that the king had taken it off his own neck and put it round Brahe's, and he particularly mentions that this second badge had a portrait of the king upon it, as it actually is represented in Dr. Crompton's portrait. Gassendi mentions an engraving of Brahe in which he is represented wearing both of the Orders so given to him.

As the inscription on Dr. Crompton's portrait says that it was painted on the completion of his 50th year, it is a representation of Tycho ten years later than the engraving by Meurs, of 1586; and the tenor of the emblems and the inscription seem to be conclusive that the picture was painted after Brahe had left Denmark, which he did (Gassendi, p. 142) about Midsummer, 1597, whence he went to Rostock, but which place he left for fear of the plague after the end of October, when by invitation he went to the Castle of Wandsburgh, near Hamburgh, to stay with his friend, Ranzovius. Immediately on his arrival there he composed a long poem in Latin, given by Gassendi at pages 143, 4, and 5, in which he complains bitterly of his country's blindness and want of appreciation of his notable services, which he lauds in a remarkable way, but not beyond their deserts. In the course of this poem are passages which justify the emblem on Dr. C.'s portrait and several lines which bear closely on the inscription on his picture.

The following may be quoted:—

Pro quibus, o Superi, mihi gratia reddita talis,
Sex ego cum natis, matreque ut exsul agam

Sum tamen haud exsul, *libertas contigit ampla;*
Exsilium in patria, verius ante tuli.
Nunc ego prosper ago; proprii nunc juris alumnus;
Nunc mihi pro patria maximus orbis adest. p. 145.

From these facts Dr. C. conjectures that the portrait was painted between the end of October and the 13th of Dec., 1597. It could not have been painted later than the 13th of December, for then the inscription would have been "anno 51 completo," and as this poem is addressed to Ranzovius, the presumption is that it was written after Brahe's arrival at Ranzovius's. And Dr. C. conjectures that the portrait may have been painted to be engraved for Brahe's "Mechanica," which he immediately began to print at Hamburgh whilst he remained at Ranzovius's, and that there may not have been time to have it engraved; or, may be, it was too self-glorious.

The residence of Ranzovius, half a mile from Hamburgh, existed till ten or fifteen years ago, when it was pulled down, and a full-length portrait of Brahe disappeared, representing him with an astronomical instrument in one hand.

Dr. C. exhibited a photograph from his picture, taken by an able photographer, Mr. White, Crescent, Salford, to whom he has given permission to dispose of impressions of it.

"Remarks on the General Affections of the Barometer noticed by Mr. J. A. Broun," by Professor B. STEWART, LL.D., F.R.S.

Mr. J. A. Broun has found as an experimental fact that simultaneous variations of the barometric pressure occur at such distant portions of the globe as to lead to the inference that the whole globe is thus affected, from which Mr. Broun infers that some other force besides gravity is concerned in these phenomena.

We know as a matter of fact that there are causes at work which give rise to electrical separation, although we may not know the precise nature of these causes. Thus evaporation goes on from the surface of the sea and of the land. Changes take place in the amount of aqueous vapour held by the air, and also probably in the molecular state of this aqueous vapour. But although we may not be able to point to the specific actions which produce electrical separation, we know that such separation implies a one-sidedness or heterogeneity; and since gravity will presumably act differently on the two things, we may probably suppose that one of the constituents which have caused this electrical separation may have a tendency to mount upwards in the atmosphere, while the other may have a tendency to move downwards. For instance, ir evaporation from the surface of the earth or sea be one cause of this electrical separation, we might imagine the land or sea to become electrified in one way, while the vapour electrified in the other direction might mount in the air, owing to its being specifically lighter. In fine, whatever be the cause of the electrical separation, we may presumably suppose that the one constituent will either remain below or find its way downwards, while the other, carrying with it its peculiar electricity, will mount upwards.

Now, may not the earth be regarded as a Leyden Jar, the sea and earth forming one coating, and the upper, rarer, and hence electrically conducting strata of air forming the other coating; and will not the tendency of the action above named be to charge the upper coating with one kind of electricity and the under with another?

Such a process would of course be continually going on, while on the other hand the earth, regarded as a Leyden Jar, would by means of thunderstorms, and possibly by other means, be continually discharging itself.

Next, let us suppose that by some extensive local circumstances a greater than usual electrical separation and charging of the Earth Jar has been going on. The effect of this local cause would, however, not be local, but would contribute to increase the charge of the earth as a whole—as one jar in act, so that the earth as a whole might, for a short period, be increasing its charge—the local charging causes being in excess of the local discharges.

Next, would not this excessive charge appear to increase the barometric pressure of the air over the whole earth ?

On the other hand, we may imagine the discharging influences to be sometimes in excess of the charging causes, and then the electrical separation of the Earth Jar would diminish, and the barometric pressure of the air appear to diminish also.

These remarks are put forward not as a formal theory, but rather with the view of inviting discussion. In considering a fact such as that brought forward by Mr. Broun, we must first endeavour to explain it by the operation of some known cause. I have therefore introduced a force which we know to exist, and a mode of operation which is not at first sight improbable. It may be thought that electrical separation can hardly be great enough to produce a sensible barometrical difference. Let this be proved, and a point will be gained by the dismissal of what seems at first sight a possible hypothesis.

Meanwhile—to bring these remarks to a practical issue—might it not be well to examine the records of atmospherical electricity corresponding to the dates of Mr. Broun's observations with the view of ascertaining whether Mr. Broun's results are in any way connected with the electrical state of the earth's envelopes.

"On the Manner in which Raindrops and Hailstones are Formed," by Professor Osborne Reynolds, M.A.

When the particles of water or ice which constitute a cloud or fog are all of the same size and the air in which they are sustained is at rest or is moving uniformly in one direction, then these particles can have no motion relatively to each other. The weight of the particles will cause them to descend through the air with velocities which depend on their diameters, and since they are all of the same size, they will all move with the same velocity.

Under these circumstances therefore the particles will not traverse the spaces which separate them, and there can be no aggregation so as to form raindrops or hailstones.

If, however, from circumstances to be presently considered, some of the particles of the cloud or fog attain a larger size than others, these will descend faster than the others, and will consequently overtake those immediately beneath them, with these they may combine so as to form still larger particles, which will move with greater velocity and more quickly overtaking the particles in front of them will add to their size at an increasing rate.

Under such circumstances therefore the cloud would be converted into rain or hail according as the particles were water or ice.

The size of the drops from such a cloud would depend simply on the quantity of water suspended in the space swept through by the drop in its descent, that is to say, on the density and thickness of the cloud below the point from which the drops started.

My object in this paper is to suggest that this is the actual way in which raindrops and hailstones are formed.

I was first led to this conclusion from observing closely the structure of ordinary hailstones.

Although to the casual observer hailstones may appear to have no particular shape except that of more or less imperfect spheres; on closer inspection they are seen all to partake more or less of a conical form with a rounded base like a sector of a sphere.

In texture they have the appearance of an aggregation of minute particles of ice fitting closely together, but without any crystalization such as that seen in the snow-flake; although the surface of the cone is striated the striæ radiating from the vertex.

Such a form and texture as this is exactly what would result if the stones were formed in the manner described above. When a particle which ultimately formed the vertex of the cone, started on its downward descent and encountered other particles on its lower face, they will adhere to it, however slightly. The mass, therefore, would grow in thickness downwards; and as some of the particles would strike the face so close to the edge that they would overhang, the lower face would continually grow broader, and a conical form be given to the mass above.

When found on the ground the hailstones are generally imperfect; and besides such bruises as may be accounted for by the fall, many of them appear to have been imperfect before reaching the ground. Such deformities, however, may be easily accounted for.

The larger stones fall faster than those which are smaller, and consequently may overtake them in their descent; and then the smaller stones will stick to the larger and at once deform them. But besides the deformation caused by the presence of the smaller stone, the effect of the impact may be to impart a rotary motion to the stone, so that now it will no longer continue to grow in the same manner as before. Hence we have causes for almost any irregularities of form in the ordinary hailstone.

It appears from the numerous accounts which have been published that occasionally hailstones are found whose form is altogether different from that described above. These, however, are exceptional, and to whatever causes they may owe their peculiarities these causes cannot affect the stones to which I am referring.

Again, on careful examination it is seen that the ordinary hailstones are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have broken off in the descent. This also is exactly what would result from the manner of formation described above.

When the particle first starts it will be moving slowly, and the force with which the particles impinge upon it will be slight and, consequently, its texture loose; as, however, it grows in size and its velocity increases it will strike the particles it overtakes with greater force, and so drives them into a more compact mass. If the velocity were sufficient the particles would strike with sufficient force to adhere as solid ice, and this appears to be the case when the stones become large, as large as a walnut, for instance.

An idea of the effect of the suspended particles on being overtaken by the stone, may be formed from the action of the particles of sand in Mr. Tilghman's sand-blast, used for cutting glass. The two cases are essentially the same, the only difference being that the hailstone is moving through the air, whereas in the case of the sand-blast, the object which corresponds to the stone is fixed, and the sand is blown against it.

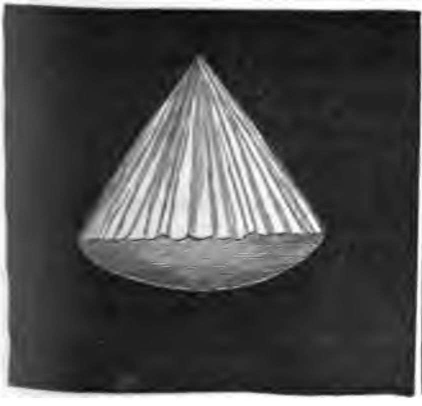
By this sand-blast the finest particles of sand are made to indent the hardest material, such as quartz or hard-steel; so that the actual intensity of the pressure between the surface of the particles of sand and that of the object they strike, must be enormous. And yet the velocity of the blast is not so much greater than that at which a good-sized hailstone descends. It is easy to conceive, therefore, that the force of the impact of the suspended particles of ice, if not much below the temperature of freezing, on a large hailstone, would drive them together so as to form solid ice. For the effect of squeezing two particles of ice together is to cause them to thaw at the surface of contact, and as soon as the pressure is relieved they freeze again, and hence their adhesion.

Nor does there appear to be any other way in which these ordinary hailstones can be formed. They are clearly not rain-drops frozen, or they would be somewhat transparent; neither are they aggregations of snow crystals. Nor can they be formed by the condensation and refrigeration of vapour on a nucleus of ice. For there is no way of getting rid of the heat which must be developed by such a process. The heat developed by the condensation of vapour one-seventh of the weight of the stone, would be sufficient to thaw the entire stone.

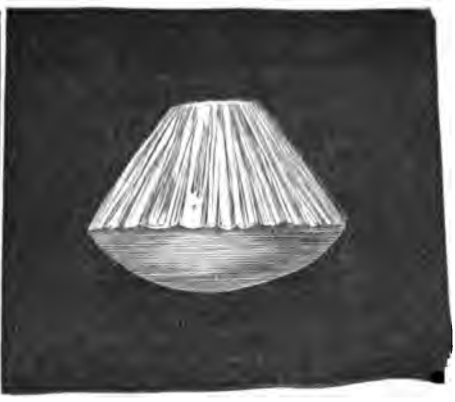
The hailstones are clearly aggregations of small frozen particles such as those which form a cloud. Nor is it possible that they can have been drawn together by some electrical attraction; for whatever such attraction, we can conceive it will not explain the conical shape of the stones or their increase in density towards their thicker sides. These clearly show that the particles have aggregated from one direction, and with an increasing force as the size of the stone has increased.

It appears as though it might be possible to make artificial hailstones. If a stream of frozen fog were driven against any small object, then the frozen particles should accumulate on the object in a mass resembling a hailstone. Not seeing my way to obtain such a stream of frozen fog, I thought it might be worth while to try the effect of blowing very finely-powdered plaster of Paris. I therefore introduced a stream of this material into a jet of steam, issuing freely into the air; (which I hoped would moisten the powdered plaster sufficiently to cause it to set firmly into whatever form it collected.) The jet was directed against a splinter of wood.

In this way I obtained masses of plaster very closely resembling hailstones. They were all more or less conical, with their bases facing the jet. But as might be expected, the angles of the cones were all smaller than those of the hailstones. Two of these figures are shown in the sketches annexed:



PERFECT HAILSTONE



BROKEN HAILSTONE.

The striæ were strongly marked, and exactly resembled those of the hailstone. The bases also were rounded. They were somewhat steeper than those of the hailstone; but this was clearly due to the want of sufficient cohesive power on the part of the plaster. It was not sufficiently wet. Owing to this cause also it was not possible to preserve the lumps when they were formed, as the least shake caused them to tumble in pieces.

I also tried a jet of the vapour of naphthaline, which at ordinary temperatures is solid, driven by means of a cross blast of air against a small object, and in this way I obtained masses closely resembling hailstones, but these also were too fragile to bear moving. At ordinary temperatures the powdered naphthaline does not adhere like ice when pressed into a lump. No doubt at very low temperatures ice would behave in the same way—that is to say, the particles would not adhere from the force of impact. Hence it would seem probable that for hailstones to be formed the temperature of the cloud must not be much below freezing point.

That the effect of the temperature of the cloud exercises great influence on the character of the hailstones cannot be doubted. And if, as has been suggested by M. L. Dufour, the particles will sometimes remain fluid, even when the temperature is as low as 0° F., it is clear that as they are swept up by a falling stone, they may freeze into homogeneous ice either in a laminated or crystalline form. Upon these questions, however, I do not wish to enter, as they have no bearing on the question as to the manner in which the mass of the stone is accumulated; and I only mention them to show that if there are unexplained peculiarities there are also causes the effects of which have not as yet been fully considered.

This view of the manner in which hailstones are formed at once suggests that raindrops may be formed in the same way; nor does there appear, on further consideration, to be any reason to suppose that such is not the case.

Of course a raindrop shows none of the structural peculiarities of the hailstone, and consequently we have not the same evidence of the manner in which raindrops are formed. But the explanation is sufficient, and there is apparently no other.

Raindrops cannot possibly have grown to the size with which they reach the earth by the condensation of the vapour of the air which they pass through, for the same simple reason as that just stated for hailstones, namely, that there is no way in which the heat developed by condensation can be got rid of. The fact that the upper regions of the air from which the drops start are colder than those through which they descend, might, as has been supposed, cause the drop to grow by condensing vapour in the air through which it passes, but only to a very small extent, and one the limit of which may be easily estimated.

Suppose the drop to start having a weight w_1 and a temperature t_1, and on reaching the earth to have a temperature t_2. Then the increase in the quantity of heat in the drop would be $(t_2-t_1)w_1$ nearly. This heat would be developed by the condensation of a weight of water $(t_2-t_1)\frac{w_1}{1000}$ nearly, so that even supposing $t_2-t_1=100°$ F., which it could not possibly be, the increase in the weight of the drop could not be one tenth.

It is obvious also that the drop would not have parted with its heat to the air it passes through, for it is assumed to be colder than this air. Therefore the only way in which it could have parted with its heat would have been by radiation. Some heat might be lost in this way but only a very small amount, and one of which an approximate estimate may be made. For after the drop had acquired a considerable size, say one hundredth of a foot in diameter, the time occupied in its descent would be very small. Assume this to be one minute, and during this time assume that the drop is 100 degrees hotter than the surrounding objects, although this is of course far beyond what could possibly be. According to the most accurate data the amount of heat it would then lose would not be sufficient to condense one hundredth of a grain of water, an altogether unappreciable amount when compared with the weight of the drop, which would be nearly the quarter of a grain.

It appears clear, therefore, that the only way in which a falling drop can grow is by the aggregation to itself of the particles of moisture in the air, and the only way in which it can encounter these is by its downward motion through this air.

Such a means of growth is amply sufficient to account for the size of rain drops or of hailstones.

If we suppose all the vapour which a body of saturated air at 60° F. would contain over and above what it would contain at 32° to be changed into a fog or cloud; then if a particle, after commencing to descend, aggregated to itself all the water suspended in the volume of air through which it swept, the diameter of the drop after passing through 2000 feet would be more than an eighth of an inch, and after passing through 4000 feet a quarter of an inch, and so on. So that in passing through 8000 feet of such cloud it would acquire a diameter of half an inch. Now, as clouds must often contain more water than what is here supposed, there is no difficulty in explaining the size of drops. The difficulty is rather the other way in explaining why the drops are not sometimes larger than they are.

There are, however, two reasons why rain drops do not acquire the full size which might be expected on the above assumptions.

In the first place, the drop will not aggregate to itself all the particles in front of it. Some of these will be swept away sideways by the diverging current of air; and the smaller the particles are, the more will this be the case. This is, of course, true for hail as well as for rain

The second reason applies only to rain, and explains why it is that hailstones sometimes acquire magnitudes never approached by rain drops.

A drop retains its form simply by the surface tension of the water, and as this is the same whatever may be the size of the drop, its power to hold the drop together diminishes as the size of the drop increases, whereas the velocity and consequent tendency of the air to disturb the shape of the drop increase with its size. Hence it must eventually arrive at such a size that it can no longer hold together, but will be blown to pieces by the rush of air past it. This action may be seen in a waterfall or a fountain, where, in passing through the air, a solid column of water is separated into drops not larger than large raindrops.

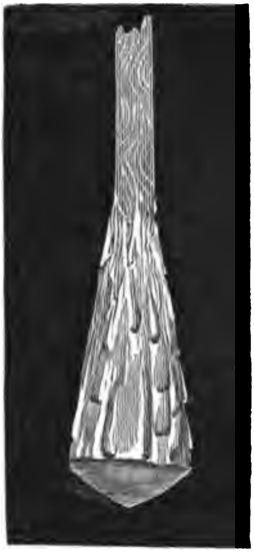
The same reasoning does not hold for hailstones, which are held together by the adhesion of the particles throughout their entire mass, and whose compactness and strength increases with their size. It is, however, the case that the smaller end of the stone, where the texture is looser, appears to be blown off in its subsequent descent, especially when the stones acquire a large size.

It seems, therefore, that, so far as the growth of a drop or a stone is concerned, the particles it overtakes in its downward path are a necessary and sufficient cause; but the origin of the drops and stones requires further explanation. Why should some of the particles in a cloud be larger than the others, as it is necessary for them to be in order that they may commence a more rapid descent?

A cloud does not always rain; and hence it would seem that in their normal condition the particles of a cloud are all of the same size and have no internal motion, and that the variation of size is due to some irregularity or disturbance in the cloud.

Such irregularity would result when a cloud is cooling by radiation from its upper surface. The particles on the top of the cloud being more exposed would radiate faster than those below them, and hence they would condense more vapour and grow more rapidly in size. They would therefore descend and leave other particles to form the top of the cloud. In this way we should have in embryo a continuous succession of drops.

Eddies in the cloud also form another possible cause of the origin of drops and stones. Whenever the direction of motion of a portion of the cloud is not straight the suspended particles will have more or less motion through the air. And if, as in an eddy, the motion of the cloud varies from point to point both in direction and magnitude, then the motion of the particles through the air will also vary, and they may overtake one another, and combining form larger particles or drops in embryo.



Whatever may be the cause of the variation in the size of the particles which form the cloud, we may know from observations on fogs that such variations do exist. In fogs we have particles of all sizes, from those which are too fine to be seen even by the aid of a microscope, and which will remain suspended for hours without any appreciable descent up to such a size that they can be easily detected with the naked eye, and descend with a very appreciable velocity so as to form a drizzle. When a coarse mist, such as this is superimposed over a fine mist, then rain must ensue if the particles are water, and hail if they are ice.

IMITATIONS IN PLASTER OF PARIS.

Although, as has been shown, a raindrop cannot add considerably to its volume by condensing the vapour from the air through which it passes; the reverse of this is not the case. The raindrop may be diminished by evaporation. Whenever a raindrop falls through dry air, that is, air of which the dew point is below the temperature, evaporation might, and would, go on to almost any extent, and the size of the drops be diminished until they entirely vanished. The heat for evaporation being supplied from the air, which would be warmer than the drop.

The case of snow differs from that of hail. The snow crystals are clearly formed by the condensation of vapour, and not by the mere aggregation of particles of ice. In this case the latent heat developed in condensation is probably dissipated by radiation, the shape and smallness of the crystals causing them to descend very slowly, and so affording time for the radiation to produce an effect.

But even in snow we see the effect of aggregation. The individual crystals never acquire a large size. But in their descent— the larger ones overtaking the smaller—they form into flakes. In this case the aggregation may be seen taking place. If when large flakes of snow are falling fast without wind the eye be fixed on a large flake as high as it can at first be perceived, and follow this flake in its subsequent descent, it may sometimes be seen to overtake another flake and combine with it, the two descending together.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 9th, 1876.

Professor W. Boyd Dawkins, F.R.S., President of the Section, in the Chair.

Mr. Percival exhibited specimens of *Campylopus paradoxus* (Wilson), a very rare moss, from a new locality, at foot of Cader Idris, North Wales, where he gathered the plant on October 5th. The specimens were barren; as are those from the two other known British localities.

The President then read an address, in which, after some introductory remarks, he said, to-night I can only bring before you a few of the more notable points which have occurred to me in the course of my reading amongst the scientific literature of the year, in scientific research, in the study of the Ancient History of Life upon the earth, and some general considerations which seem to me opportune under the circumstances of the time.

1. *The Deep Sea Dredgings.*

Two most important additions have been made to our knowledge of recent zoology, by the publication of Wallace's Geographical Distribution of Animals [2 vols. 8vo., 1876.]; and of the Reports of "The Challenger," in the proceedings of the Royal Society. To the former of these it is necessary merely to allude, for it is in everyone's hand. The latter is peculiarly valuable, because it proves the existence in the deep sea fauna of many forms which occur fossil in our European rocks. According to Mr. Moseley, out of rty-two true corals brought up in the dredge, no less than twenty genera occur in the fossil state, two extending as far back as the Jurassic strata, and eight being Mesozoic [Proceed. R.S., Feb. 26, 1876. xxiv. No. 170, p. 566].

From Mr. Murray's Report [op. cit., p. 471] we gather most valuable data for the geologist. The depths of the sea are covered with the following deposits, which have been formed out of the reach of the land.

1. *Globigerina ooze.*
2. *Radiolarian ooze.*
3. *Diatomaceous ooze.*
4. *The Red and Grey Clays.*

The first of these deep sea deposits is represented on the land by the chalk; the second and third by the siliceous and chertsy accumulations in many calcareous strata, such for example as the mountain limestone of Derbyshire; while the last throws light on the mode in which some of the red, purple, and grey clays and slates have been probably formed, such as the Cambrian and in part lower Silurian strata, which are viewed by Prof. Ramsay as having been formed in land-locked seas, or even freshwater lakes, [Quart. Journal Geol. Soc. Lond., xxvii. pp. 250 and 253.] In a word, the results of the Challenger expedition in my belief will be if possible of greater value to the geologist and palæontologist than to the student of zoology.

2. *Professor Huxley on Ceratodus.*

Among the more important special contributions to recent zoology is the first instalment of an essay on *Ceratodus Forsteri,* the extraordinary 'baramunda' of Queensland, by Professor Huxley [Proceed. Zool. Soc. 1876, 4 January], in which he takes up the investigation where it had been dropped by Dr. Günther in 1871 [Phil. Trans. 1871, Part ii]. From the study of the brain, skull, and limbs, he concludes that it occupies a curiously central position among the Ichthyopsida, being allied on one side to the Amphibia, on another to the Chimæroid fishes and the sharks, and on yet the order Odontornithes, or birds with well defined teeth in their upper and lower jaws. One genus is a large carnivorous diver with simple conical teeth imbedded in a groove in the dentary bone as in some reptiles. Each tooth possesses a crown covered with enamel and a stout conical fang. The sternum is without a keel, and the vertebræ are of the usual avian type. It was among the largest of aquatic birds, being from five to six feet from bill to feet.

The other genus is Ichthyornis, a creature about the size of a pigeon, possessed of a series of simple conical teeth implanted in well defined sockets in upper and lower jaws, of a keeled sternum, and of biconcave vertebræ. It was of aquatic habit, and capable of prolonged flight. The combination of a series of conical teeth with a keelless sternum in the former and with biconcave vertebræ in the latter points distinctly in the direction of the Reptilia, which stand next to the Aves in classification [Sauropsida].

5. *These Intermediate Forms may be accounted for on the Theory of Evolution.*

Before these discoveries were made the existence of intermediate forms between the Aves and Reptilia had been fully recognised. The Deinosauria, the gigantic Iguanodon, Megalosaurus and their allies, in the structure of their pelves, their sacra, and their hind limbs, were unlike any living reptiles, and allied to the birds. On the other hand the Archæopteryx from the Solenhofen slate, a deposit of middle oolitic age, presents us with a bird possessing a long tail composed of separate vertebræ and metacarpals free and disunited, characters which are now reptilian and not avian. Thus we had proof of the existence of Mesozoic reptiles reaching forward, so to speak, in the direction of living birds, and fossil birds pointing in the direction of the reptilia. In addition to this, we now know that certain of the Mesozoic reptiles possessed characters peculiar to the living mammalia. Why should this be? Why were the Mesozoic reptilia more richly endowed than the living, and why should the Mesozoic birds possess reptilian structures? The idea which I have formed for myself on this point is that the high organisation of the reptilia quoted above has been inherited by the Aves and Mammals, and that the present less richly endowed reptiles are to be looked upon as younger sons cut off from the estates of their ancestors by a natural law of entail. That the aves and mammalia have actually been derived from a reptilian stock seems to me to be probable from the changes which the embryos undergo.

The question so often triumphantly asked by the adversaries of the doctrine of evolution, *how* is this or that change from one form into another produced, seems to me to be altogether idle. We know very little of the how or the why of any natural process. It is a fact that a grain of wheat will produce a blade, and ultimately repeat itself many times over; although we cannot tell what are the precise forces which regulate its form and development. It is sufficient for me to know that these changes actually take place, without knowing how they have been produced. They are altogether unintelligible to me without a belief in evolution, of which the very least that can be said is that it is a good working hypothesis, which is held consciously or unconsciously by most of the leading naturalists of the day.

6. *The Antiquity of Man.*

The exploration of the late Tertiary deposits, and more particularly of the ossiferous caverns, has made important additions to our knowledge of Man, in Europe, during the last two years. The incised figures of rein-deer and horses found in the cave of Kesslerloch [Excavation at the Kesslerloch, by Conrad Merk, translated by Lee, 1876] proves that palæolithic tribes, allied probably to the Eskimos formerly, lived in Switzerland along with the extinct mammalia, while the discoveries at Cresswell, on the borders of Derbyshire and Nottinghamshire, show that they inhabited the caves to the east of the Pennine Chain, in the Pleistocene age [Quart. Journ. Geol. Soc., Lond., xxxii., pp. 245—259]. So that now we have proof that the palæolithic man of the caves wandered from the Pyrennees and Alps as far north as Derbyshire and Belgium, and from the Atlantic coast as far to the east as Schussenreid, in Würtemburg. In the upper pleistocene stratum, at Cresswell, we have also decided traces of the presence of a higher type of hunter than in the lower one, so that man, even in the palæolithic age, was a creature of progress. The evidence as to their relation to the Glacial Period in every case which I have investigated, is unsatisfactory. In the celebrated Victoria Cave, in which the glacial age of the clay covering up the stratum with the remains of pleistocene animals and the fibula determined by Professor Busk to be human, seems as doubtful to me as to Professor Hughes, who has carefully examined the evidence on the spot. It is however considered glacial by Mr. Tiddeman, and is quoted by Mr. James Geikie as proof of the interglacial age of palæolithic man in this country. It is extremely difficult to bring the complicated phenomena of the glacial deposits into relation with the ossiferous deposits in caves and river-valleys, which are equally complicated, and their point of contact offers a large field for speculation. Fixing my eyes upon the pleistocene fauna only, I find that that portion of it to which man belongs—the arctic division—arrived in Britain before the deposit of the boulder clays, and lived here afterwards, and that therefore there are *a priori* grounds for supposing that man was also here in the preglacial age; but actual proof on the point seems to me to be wanting unless it be offered by the fluviable gravels with palæolithic implements of Brandon, which according to Mr. Skertchley [Nature xiv. 448] are older than the upper boulder clay of East Anglia, considered by Mr. Tiddeman [Nature xiv. 505] to be the equivalent of the lower boulder clay of Lancashire. This discovery, however, needs verification.

7. *The Position of Museums in Britain.*

I come now to a subject which to my mind touches the interests of this Society, because it stands in close relation to the advancement of the study of the history of the earth and of its inhabitants in Manchester — the subject of Museums. It seems to me that it is necessary for the student of nature in the highest and widest sense to have access to well arranged collections of objects which stand to them in the same position as books to the ordinary student, and to have in addition laboratories, which are the equivalents of reading rooms. Without these their labours may be unpractical and sporadic. There ought also to be a teaching staff to give system and direction to what otherwise would be unfruitful efforts. This ideal is only carried out in a few places in this country, among which I would mention more particularly the Museums at Oxford and Cambridge. From my experience of those abroad, I turn from those of our own country with feelings of envy and regret. Here a museum is frequently a large sort of advertising bazaar, or a receptacle for miscellaneous curiosities unfitted for a private house, or it is composed of an accumulation of objects valuable in themselves, but valueless for all practical purposes, because they are crowded together, or stowed away for want of room. They are generally under-manned, starved for want of funds, largely dependent on casual benevolence, or a burden on the scant resources of the various societies. On the Continent, in America, and in Australia, they are as a rule well officered, well arranged and not dependent on private resources for their sustenance. That our museums should be allowed to be such a striking contrast to those of our neighbours and kinsmen is a most singular oversight in the richest and, as we sometimes fancy ourselves to be, the most practical people in the world.

With regard to the arrangement of subordinate parts in a museum, that which is now being carried out in the new Imperial Museum, at Vienna, under Dr. Hochstetter, seems to me the best; to form one lineal series, inorganic objects forming the base, then Palæontological specimens, illustrating the life which has been, and leading up to the illustrations of the life which is now on the earth, Botany, Zoology, Anatomy, and the like. When this is completed, the Museum at Vienna will present a more perfect and complete history of the knowledge of the earth and its inhabitants than has, as yet, been presented.

In the City of Lyons, which in its commercial aspects resembles Manchester, the collections are lodged in a magnificent building—the Palais des Beaux Arts—supported by the municipality, and are being largely increased by the contributions of local naturalists, who have banded themselves together for that purpose, under the title of "Les Amis des Science Naturelles." I hope to live to see the day when a plan similar to that of the Museum in Vienna will be carried out in Manchester, and when our Museum will be a centre around which our naturalists will rally, as at Lyons.

8. *A Museum necessary in Manchester.*

It is certainly very strange that in a city where various branches of knowledge have been endowed by private munificence for the common weal, a Museum should be wanting, which ought to be a powerful agent in the general education of the people; and not merely, as is sometimes the case, the spoil and prey of a few specialists. Museums are rapidly becoming as indespensible in the advancement of culture as libraries, and are so recognised in all civilised countries except our own. It is a poor consolation to know that in this particular we are not worse off than the majority of our countrymen, although we are worse off than Liverpool, and many other places which I could mention. We ought to be much better off. I trust that in no far distant future this need may be satisfied by the raising of buildings worthy of the dignity of Manchester, and by the placing in them of our valuable collections, arranged not merely for the advantage of the student but of the general public.

9. *Collections made by the German Government.*

There is one point in which the British Government may learn a lesson from the German. When I was in Berlin this autumn I had the pleasure of meeting gentlemen who had been sent by the latter to make collections in the Americas, in India, and in the China seas, and I saw a valuable collection made by German cruisers in the Pacific. Why should not our ships of war, which are to be found in every sea, have orders also to bring home collections from distant stations, and why should not we send out travellers with the same object? With our navy and our wandering instincts, we ought rapidly to outstrip any rivals, and that at a comparatively small expenditure of money.

10. *The Work of Scientific Societies in General Education.*

In conclusion I would merely allude to the endowment of original research, which has been before the notice of the public for the last three years. In the discussion of this most difficult question the influence of societies such as our own has been for the most part ignored. The endowment, which, to my mind is the most important for the promotion of original research' is not the gift of money, but that approbation of good work done by any worker, which is afforded by the learned societies. In this particular they are doing incalculable good in the general work of education, good which will be recognised by the future writers of history, but which, so far as I know up to the present time, has only been fully acknowledged by the able and brilliant historian of "The English People."

Ordinary Meeting, November 14th, 1876.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

The President said that with respect to the meteor described by him at the meeting of the Society on the 17th ulto. he had received a letter from Mr. John Petrie, dated Broomfield, Rochdale, 4th November, as follows:—"Seeing an account of the meteor observed by you in Douglas Bay on the 15th of August, I take the liberty of saying that I saw the same meteor while crossing a field here. I at first thought that it was a rocket, but the slow, steady, and almost horizontal course it took convinced me it was a meteor. I described it to my family on entering the house, and your description tallies with what I saw and described."

The President also said that during the last Session one of the Vice-Presidents, Dr. R. Angus Smith, F.R.S., brought before the Society an interesting paper on the Eucalyptus near Rome. About a year and a half since he (the President) planted in his garden at Douglas in the Isle of Man, within fifty feet of the sea and six feet above high water mark, a small plant of the Blue Gum tree (Eucalyptus globulus). During the last winter the stem died down to within a foot of the ground. In the month of April it began to grow vigorously, and by the 5th of October it had reached the height of 8ft. 3in., and between the last named date and the 8th of November it had attained to 8ft. 8in., or a total length of 7ft. 8in. The situation where the tree grew was sheltered from the winds by high walls, and the soil was formed of the *debris* of Manx schist, which he believed contained a considerable amount of potash, but it had received no manure.

"Notice of a passage in Clement of Alexandria on the Origin of certain Arts and Customs, and their Introduction into Greece," by WILLIAM E. A. AXON, M.R.S.L., &c.

We know so little of the origin of various arts that a few moments may not be misemployed in noticing a passage in Clement of Alexandria which has not, to my knowledge, been referred to in this connection. I have not, however, had an opportunity of referring to Isidore. Clement, before his conversion, had become familiar with the learning and philosophy of his age, and his interest in Christianity did not exclude from sight his former liberal studies. In the Stromata he has an essay to show the obligation of Greek philosophy to the despised barbarians. This he follows by another showing "that the inventors of other arts were mostly barbarians." The passage is thus translated by the Rev. William Wilson, M.A.:

"And barbarians were inventors not only of philosophy, but almost of every art. The Egyptians were the first to introduce astrology among men. Similarly also the Chaldeans. The Egyptians first showed how to burn lamps, and divide the year into twelve months, prohibited intercourse with women in the temples, and enacted that no one should enter the temples from a woman without bathing. Again, they were the inventors of geometry. There are some who say that the Carians invented prognostication by the stars. The Phrygians were the first who attended to the flight of birds. And the Tuscans, neighbours of Italy, were adepts at the art of the Haruspex. The Isaurians and the Arabians invented augury, as the Telmesians divination by dreams. The Etruscans invented the trumpet, and the Phrygians the flute. For Olympus and Marsyas were Phrygians. And Cadmus, the inventor of letters among the Greeks, as Euphorus says, was a Phœnician; whence also Herodotus writes that they were called Phœnician letters. And they say that the Phœnicians and the Syrians first invented letters; and that Apis, an aboriginal inhabitant of Egypt, invented the healing art before Io came into Egypt. But afterwards they say that Æsculapius improved the art.

Atlas the Libyan was the first who built a ship and navigated the sea. Kelmis and Damnaneus, Idæan Dactyli, first discovered iron in Cyprus. Another Idæan discovered the tempering of brass; according to Hesiod a Scythian. The Thracians first invented what is called a scimitar (ἅρπη), it is a curved sword, and were the first to use shields on horseback. Similarly also the Illyrians invented the shield (πέλτη). Besides, they say that the Tuscans invented the art of moulding clay; and that Itanus (he was a Samnite) first fashioned the oblong shield (ϑυρεός). Cadmus the Phœnician invented stonecutting and discovered the goldmines on the Pangæan mountain. Further, another nation, the Cappadocians, first invented the instrument called the nabla and the Assyrians in the same way the dichord. The Carthaginians were the first that constructed a trireme; and it was built by Bosporus, an aboriginal. Medea, the daughter of Æetas, a Colchian, first invented the dyeing of hair. Besides, the Noropes (they are a Pæonian race and are now called the Norici) worked copper, and were the first that purified iron. Amycus the king of the Bebryci was the first inventor of boxing-gloves. In music, Olympus the Mysian practised the Lydian harmony; and the people called Troglodytes invented the sambuca, a musical instrument. It is said that the crooked pipe was invented by Satyrus the Phrygian; likewise also the Diatonic harmony by Hyagnis, a Phrygian too; and notes by Olympus, a Phrygian; as also the Phrygian harmony, and the half-Phrygian and the half-Sydian by Marsyas, who belonged to the same region as those mentioned above. And the Doric was invented by Thamyris the Thracian. We have heard that the Persians were the first who fashioned the chariot, and bed and footstool, and the Sidonians the first to construct a trireme. The Sicilians, close to Italy, were the first inventors of the phorminx, which is not much inferior to the lyre. And they invented castanets. In the time of Semiramis queen of the Assyrians, they relate that linen garments were invented. And Hellanicus says that Attosa queen of the Persians was the first who composed a letter-

These things are reported by Scamo of Mitylene, Theophrastus of Ephesus, Cydippus of Mantinea, also Antiphanes, Aristodemus, and Aristotle, and besides these, Philostephanus, and also Strato the Peripatetic, in his books *concerning inventions.* I have added a few details from them, in order to confirm the inventive and practically useful genius of the barbarians, by whom the Greeks profited in their studies. And if anyone objects to the barbarous language Anacharsis says, 'All the Greeks speak Scythian to me.' It was he who was held in admiration by the Greeks who said, 'My covering is a cloak; my supper, milk and cheese.' You see that the barbarian philosophy professes deeds, not words. The apostle thus speaks: 'So likewise ye, except ye utter by the tongue a word easy to be understood, how shall ye know what is spoken? for ye shall speak into the air. There are, it may be, so many kind of voices in the world, and none of them is without signification. Therefore if I know not the meaning of the voice, I shall be unto him that speaketh a barbarian, and he that speaketh shall be a barbarian unto me,' and 'Let him that speaketh in an unknown tongue pray that he may interpret.' Nay more, it was late before the teaching and writing of discourses reached Greece. Alcmæon, the son of Perithus, of Crotona, first composed a treatise on nature. And it is related that Anaxagoras of Clazomenæ, the son of Hegesibulus, first published a book in writing. The first to adapt music to poetical compositions was Terpanda of Antissa; and he set the laws of the Lacedæmonians to music. Lasus of Hermione invented the dithyramb; Stesichorus of Himera, the hymn; Alcman the Spartan, the choral song; Anacreon of Teos, love songs; Pindar the Theban, the dance accompanied with song. Timotheus of Meletus was the first to execute those musical compositions called νόμοι on the lyre, with dancing. Moreover, the iambus was invented by Archilochus of Paros, and the choliambus by Hipponax of Ephesus. Tragedy owed its origin to Thespis the Athenian, and comedy to Susarion of Icaria. Their dates are handed down by the grammarians. But it were tedious to specify them accurately; presently, however, Dionysus on whose account the Dionysian spectacles are celebrated, will be shown to be later than Moses. They say that Antiphon of Rhamnusium, the son of Sophilus, first invented scholastic discourses and rhetorical figures, and was the first who pled causes for a fee, and wrote a forensic speech for delivery, as Diodorus says. And Apollodorus of Cuma first assumed the name of critic, and was called a grammarian, Some say it was Eratosthenes of Cyrene who was first so called, since he published two books which he entitled *Grammatica.* The first who was called a grammarian, as we now use the term, was Praxiphanes, the son of Disnysophenes of Mitylene. Zeleucus the Locrian was reported to have been the first to have framed laws (in writing). Others say it was Menos the son of Zeus, in the time of Lynceus. He comes after Danaus, in the eleventh generation from Inachus and Moses, as we shall show a little further on. And Lycurgus, who lived many years after the taking of Troy, legislated for the Lacedæmonians a hundred and fifty years before the Olympiads. We have spoken before of the age of Solon. Draco (he was a legislator too) is discovered to have lived about the three hundred and ninth Olympiad. Antilochus, again, who wrote of the learned men from the age of Pythagoras to the death of Epicurus, which took place in the tenth day of the month Gamelion, makes up altogether three hundred and twelve years. Moreover, some say that Phanothea, the wife of Icarius, invented the heroic hexameter, others Themis, one of the Titanides. Didymus, however, in his work On the Pythagorean Philosophy, relates that Theano of Crotona was the first man who cultivated philosophy and composed poems. The Hellenic philosophy then, according to some, apprehended the truth accidentally, dimly, partially; as others will have it, was set agoing by the devil. Several suppose that certain powers descending from heaven inspired the whole of philosophy. But if the Hellenic philosophy comprehends not the whole extent of the truth, and besides is destitute of strength to perform the commandments of the Lord, yet it prepares the way for the truly royal teaching, and moulding the character, and fitting him who believes in Providence for the reception of the truth."—*Miscellanies, Book I., Chap. XVI., Writings of Clement of Alexandria—Ante-Nicene Christian Library. Edinburgh, 1868.*

To compare this with the well known passage in Pliny and to annotate it in a satisfactory manner would extend this communication to an unreasonable extent. The task is therefore left to other opportunities or abler hands.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 6th, 1876.

CHARLES BAILEY, Esq., in the Chair.

"Wealden Fossils from Columbia, S. America," by JOHN PLANT, F.G.S.

The author stated that in the republic of Columbia, in the mountain ranges about Honda on the Upper Magdalena, having an average altitude of about 9,000 feet, mines of silver are worked, and emerald mining is carried on to a large extent in metamorphic rocks of Palæozoic age. Of the existence of fossiliferous rocks of Wealden age lying upon these older rocks at such a high elevation as 8,000 or 9,000 feet he has had proof in a few fossils sent by his friend Mr. E. Williamson from Honda, who has been five years at the silver mines.

The first is an Ammonite, or the cast of one in a black slatey shale, very much like shale found in the Silurian district of N. Wales. This Ammonite is 3 inches long, strongly whorled with a row of tubercles near the outer margin. The last whorl is very large and carinated. The "facies" of the species is decidedly allied to British Cretaceous and Wealden Ammonites. The second specimen would readily be mistaken for a piece of the well known water limestone called the Petworth and Purbeck marbles from Sussex—which are of Wealden age. It is made up of a small species of Paludina, imbedded in a blackish-brown matrix. The Paludinæ are much broken, but the few perfect ones exhibit no specific difference in form to those in the English Wealden marbles. It is remarkable that shaley limestones similar in all respects as to origin and species should have been deposited in two localities so widely apart as Kent and the Cordilleras of Columbia.

"The Raised Beaches of County Antrim, their Molluscan Fauna, and Flint Implements," by **Mark Stirrup**, F.G.S.

Among the clearest evidences of the slow upheaval of land and the consequent change in the relative level of land and sea, may be cited raised beaches.

They occur in many places around our shores, often at some little distance inland, and at various elevations above the present sea level,

Their height above the sea level is a rough gauge of their relative age, and their molluscan fauna of the climatical conditions then prevailing.

On the coast of county Antrim, north east of Ireland there are several examples differing alike in position and antiquity.

These Post-Tertiary deposits have been long known, and have been noticed by several writers, as the Rev. Dr. Grainger of Belfast, Professor Hull, and others.

The fine section which was exposed at Larne, by the cutting of the road to the harbour through the deposit, and at which I spent a short time last year, had almost entirely disappeared on my visit in April of this year. The ground had been cleared to build a large corn warehouse for the accommodation of the shipping of the thriving port of Larne. The mass had apparently been carted to some adjoining land. The flint gravels, of which a large portion of it consists, have been long used for road metal, for which it forms an excellent material.

The raised beach between Ballygally and Glenarm is also being rapidly destroyed for the same purpose.

The two deposits I have just mentioned, although differing slightly in their molluscan fauna, both from their position above the present sea level and the general facies of the shells therein, may, I think, be referred to the same period of depression of the land. The shells, although mostly of species now found living in the adjoining sea, present a northern or boreal aspect; some indeed are typical of glacial beds.

In the naming of them I have had the kind assistance of Mr. R. D. Darbishire, F.G.S.

My list must not be considered a complete one, as it contains only the names of those species I have myself obtained and of which I exhibit specimens; but it will give an idea of the richness and character of the deposit.

The raised beach at the Curran of Larne is an extensive deposit through which the railway and road to the harbour are cut, and is from 10 to 20 feet above high water mark.

It consists of stratified sand and gravel; the stratum of sand 2 to 3 feet thick forms the lower part, while the remainder is made up of firmly compacted gravel and sand; the stones, some of which are of considerable size, are principally of flint, hard chalk, trap, and lias; rolled specimens of Gryphœa incurva are also met with.

In the sandy layer near the base, I found Tapes pullastra and numerous specimens of the young of Lucina borealis, with both valves in apposition, showing that they had lived and died on the spot.

Most of the shells are blanched and fragile; some of them falling into powder in the act of extraction.

To shew the chemical decomposition going on, two very curious examples were exhibited of a Littorina and Natica, in which the outer layer of the shells has been decomposed, leaving only the thin inner ones as casts. Some of the smaller shells are often preserved from injury by being enclosed in the mouth of some of the larger species of univalves.

No. 1.

Raised Beach at the Curran, Larne, 10 to 20 feet above high water.

Anomia ephippium
Lucina borealis
Cardium edule
„ pygmæum
Tapes pullastra
Mactra (fragment)
Syndosmya alba
Patella vulgata
„ lævis
Trochus cinerarius
„ umbilicatus
Littorina littoralis
„ rudis
„ littorea
Purpura lapillus
Buccinum undatum
Nassa reticulata
Cliona borings
Natica } Outer layer of shell decomposed, leaving only the inner layer
Littorina }

Going northward along the coast road from Larne and at about two miles past Ballygally in the townland of Ballyrudder, abutting against the cliffs we have the remains of another raised beach. It is stratified, with evidences of current bedding, the lower portion being of sand and small pebbles 12 to 15 feet thick, with here and there a little patch of clay, over this lies a stratum of larger water-worn stones or gravel. Many of the shells are in a fragmentary condition and are not now found plentifully. This deposit is about 15 to 20 feet above high water.

The shells seem to bear a more arctic type than those found at Larne and the absence of Patellæ may be noticed; probably the rocks here do not form a suitable habitat for them.

No. 2.

Raised Beach on the landward side of the coast road between Larne and Glenarm, in the townland of Ballyrudder, 15 to 20 feet above high water.

Mytilus (fragment)
Leda pernula
Astarte elliptica
„ compressa
Tellina solidula
Mactra solida (fragment)
Puncturella Noachina
Trochus zizyphinus
Natica (fragment)
Littorina littoralis
„ rudis
„ littorea
Purpura lapillus
Buccinum undatum
Trophon clathratus
Mangelia turricula
Fusus antiquus (fragment)
Balanus

In addition to these older raised beaches there are along the shores of Belfast Lough and at points along the coast road from Larne northwards, remains of gravel beaches of a much more recent origin as shown by their slight elevation above the present high water mark and the better state of preservation in which the shells are found.

From Larne to Waterloo, a distance of about two miles, this raised beach may be traced in some degree continuously. The present sea beach is a sloping one covered with the gravel of the older beach, which is about 5 to 6 feet above the sea level, and is being wasted yearly.

The lowest portion visible seems to be all sand with occasional shells; the whole deposit of sand and gravel being about 3 to 4 feet thick.

In the stratum of sand occur at intervals thick beds of shells consisting for the most part of Patellæ, Littorinæ and Trochi. This band of shells may be traced for several yards at a time, and then dies away.

This old beach is suffering from the inroads of the sea, and the shore is strewn with its *débris;* shells that have been washed out may be found mixed with recent ones.

A small detached portion of this old beach which rested on the lower lias rocks at Waterloo had entirely disappeared between my visit of last year and this. It would seem therefore that the land is either undergoing a slight depression or that the currents which run along the coast are altering.

Near Larne, at the junction of the band of sand with the gravel I found firmly imbedded in the sand, a fragment of bone and a tooth which has been identified by Professor Boyd Dawkins as a tooth of Bos longifrons.

No. 3.

Raised Beach along the coast between Larne and Waterloo, 5 to 10 feet above high water mark.

Patella vulgata
,, lævis
Pectunculus glycymeris
Scrobicularia piperata (?)
Trochus cinerarius
Littorina littoralis
,, rudis
,, littorea
Purpura lapillus
Tooth of Bos longifrons

No. 4.

Shore at Kilroot—shells of old sea beach, mixed with recent ones.

Cardium edule, recent
Mytilus edulis
Tellina solidula, recent
Artemis exoleta (with Serpulæ),
 recent
Patella vulgata
Trochus cinerarius
Littorina littoralis
„ littorea
Rissoa ——
Purpura lapillus
Buccinum undatun.
Nassa reticulata

At Portrush in the extreme north of Antrim another interesting deposit is found composed entirely of shells and shelly sand, and it is remarkable for the number of species it has afforded to collectors.

It is situated in the wave worn clefts of the rocks near the inlet called Port Scaddan and is about 10 feet above high water.

The road near the shore goes over a portion of the deposit and it is dug out in shallow pits and used for garden walks instead of gravel.

No. 5.

Deposit of shells, sand, and broken shells, at Port Scaddan, Portrush, 5 to 10 above high water.

Anomia ——
Mytilus edulis
Nucula nucleus
Mactra subtruncata
Patella vulgata
„ lævis, abundant
Fissurella Græca
Trochus cinerarius, very abundt.
„ zizyphinus, fairly „
Lacuna vincta
Littorina littoralis, vy. abundt.
„ rudis
„ littorea, abundant
Rissoa parva
Purpura lapillus, very abundant
Murex erinaceus
Nassa incrassata
Cypræa Europæa, common
Serpula
Balanus crenatus

Flint Implements.

In the older raised beach at Larne as well as in the more recent flint gravels along the coast at Kilroot, Ballygally, &c., are found rude flint implements of a palæolithic type and not distinguishable in make and form from the flint flakes found in the river gravels of England and France.

They occur in great numbers and are usually of a whitish colour, with a porcellaneous glaze upon them, a good evidence of their antiquity which no fresh fractured flint can simulate. This appearance is no doubt due to long exposure and decomposition of the outer crust of the flint.

The reason for their great abundance in these localities may be due to the unlimited supply of the raw material which is furnished by the flint gravels which line the coast

What are known as scrapers or thumb flints are also found as well as the flint cores from which the flakes have been struck.

If these flint implements bear on their face the evidences of design and human workmanship, and of that, I think, there will be no question, as they bear certain characters well understood by experts, then we have evidence of man in a rude state of existence living in Ireland when the land was much more depressed beneath the waters of the ocean than now. Three of the flint flakes exhibited I extracted from about the middle of the consolidated bed of sand and gravel which constituted the older raised beach at Larne; therefore this thick deposit must then have been in process of formation around the ancient shore of Antrim.

Similar elevations of land as shown to have taken place in Ireland since the human period, have also occurred along the western shores of England and Scotland, as instanced by the same phenomena and the fact mentioned by Mr. Geikie in his great "Great Ice Age" of several canoes having been dug out of the 25 feet terrace along the estuary of the Clyde.

A general elevation of land may have been synchronous on both sides of the channel, and Professor Hull, in a paper read before the British Association at Brighton in 1872, suggested the correlation of the raised beaches of Ireland and the British coasts as desirable.

General Meeting, November 28th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., Vice-President, in the Chair.

Mr. Walter Barrett, of Higher Broughton; Mr. James Smith, of Cleveland Road, Crumpsall; and Mr. Thomas Parry, of Ashton-under-Lyne, were elected Ordinary Members of the Society.

Ordinary Meeting, November 28th, 1876.

EDWARD SCHUNCK, Ph.D., F.R.S., Vice-President, in the Chair.

"*The Radiometer.*"—Mr. HARRY GRIMSHAW, F.C.S., communicated the following summary of an extract from the "Panorama of Science and Art," published by Nuttall, Fisher, and Dixon, 1813, 2 vols.

"After alluding to Boerhaave's experiment on the influence of the 'burning glass' on the motion of the 'compass,' the extract goes on to describe a radiometer constructed by Mitchell, which seems to have been constructed as follows:—A thin plate of copper 1 inch square was attached to one end of a fine 'harpsichord' wire 10 inches long. This was balanced on an agate suspension, and the little copper plate was counterpoised by a grain of shot at the other extremity of the wire.

"As a result of experiments with the instrument, it was found that the influence of the rays of the sun focussed by a concave mirror 2 feet in diameter, caused a revolution of 1 millionth of an inch in a second.

"The instrument was protected by some sort of glass shade.

"The same motion was produced in a vacuum."

"Note on a Manganese Ore from New South Wales, and on a Specimen of Native Silver from New Zealand," by M. M. PATTISON MUIR, F.R.S.E.

I lately received a small specimen of manganese ore taken from a very large deposit near Bathurst, N.S.W. The ore is said to constitute the greater part of a considerable mountain in that neighbourhood.

The sample was greyish-black in colour; when broken it exhibited an ill-defined crystalline structure, and showed patches of dark brown or black intermingled with others of dark steel-grey colour.

When heated in a small glass tube a considerable quantity of water was evolved.

The amount of manganese dioxide was determined by the oxalic acid method of Fresenius and Will: the total quantity of manganese was also determined by precipitation with bromine water, after removal of ferric and aluminic oxides: the excess of manganese above that required for the formation of the amount of dioxide found to be present was calculated to protoxide.

The following are the results of the analysis.

Manganese Dioxide	78·72	per cent.
„ Protoxide	3·66	„
Ferric and Aluminic Oxides ...	6·50	„
Silica	5·80	„
Moisture	4·75	„
	99·43	

If the percentage amount of manganese dioxide be calculated on the dried specimen, it is found to amount to 82·21.

Traces of zinc and of sodium were also found.

The specimen of native silver, the analysis of which is appended, was sent to me from the Thames Goldfield, New Zealand. It presented the appearance of arborescent crystals radiating outwards from a central mass at acute angles. A very small quantity of crystalline quartz could be seen in the cavities of the mass.

The silver was estimated by precipitation as chloride; the copper, mercury, &c., were precipitated by means of sulphuretted hydrogen; the copper was separated by solution in nitric acid, and estimated in this solution by the colorimetric method depending upon the use of potassium ferrocyanide described by Carnelley (Proc. Manchester Lit. and Phil. Soc., 1875-76, p. 24). The iron was precipitated by means of ammonium sulphide, the precipitate was dissolved in dilute hydrochloric acid, and the quantity of iron estimated by Carnelley's colorimetric method (Proc. Lit. and Phil. Soc., vol. v., p. 346).

The sulphide of mercury was dissolved in aqua regia, and an attempt was made to estimate the quantity of the metal by Hannay's volumetric method (Chem. Soc. J. [2] xi. 565). The quantity present was, however, too small to admit of estimation by this method.

The mercury was approximately estimated by comparing the depth of colour produced in the liquid on the addition of sulphuretted hydrogen water with that produced in an equal bulk of pure water to which a known volume of standardised mercuric chloride solution had been added. This method did not give very satisfactory results: it was found that the shades of colour in the two liquids were not exactly the same. For this reason the quantity of mercury stated below must be regarded as approximative only.

Silica and Gangue......	1·93	
Silver....................	97·05	
Mercury..................	0·28	
		99·26
Copper	0·00005	
Iron	0·00109	

Very minute traces of bismuth were also detected.

Mr. Muir also exhibited a number of new salts of Bismuth, including several chromates, oxybromides, ferrocyanides, &c., likewise specimens of terpenes, &c., from essential oil of sage.

Mr. W. H. COOKE read an "Extract from an Autobiographical Memorandum by the late Mr. G. Finlay, of Athens."

Mr. R. D. DARBISHIRE, F.G.S., then exhibited and described a Selected Series of Neolithic Implements from Greece, from Mr. Finlay's Collection.

Ordinary Meeting, December 12th, 1876.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that he had received a letter from Mr. N. Staples, at Lissan, Cookstown, Ireland, dated December 4th, on the subject of the meteor of the 15th August last, as follows: "As I notice in the paper that you observed a meteor on the night of the 15th August when in the Isle of Man, I beg to inform you that a meteor was observed in Cookstown (about longitude W. 6° 45′) on the night of Tuesday, the 15th, about 9.45 local time (time observed probably not exact) passing over from S.E. to N.W. It was described to me *as lighting up the whole street, colour reddish green.*" This shows that it was observed from Rochdale in the east to Cookstown in the west, a distance of nearly two hundred miles.

"The Lowest Amounts of Atmospheric Pressure during the last Sixteen Years, as observed by Thomas Mackereth, F.R.A.S., F.M.S."

The following are the lowest pressures of the atmosphere observed at Eccles during the last 16 years. Each reading of the barometer has been reduced to 32° Fahr. and the height at sea-level. The weekly means are for the whole week in which the lowest reading was registered. No minimum reading has been considered which, at sea-level, would be 28·6 inches.

Year.	Month.	Date.	Time of Observation.	Lowest reading reduced.	Mean Height for the week.
				Inches.	Inches.
1865	January.........	14	9 a.m.	28·360	29·277
1867	„	8	„	28·558	29·228
1872	„	24	„	28·454	29·253
1873	„	19	11.30 p.m.	28·327	29·127
1876	March	10	9 a.m.	28·568	29·173
1876	December......	4	„	28·379	29·143

The lowest reading for January 19th, 1873, was the absolute minimum, as the barometer was read every half hour till the minimum had been reached and the mercury began to rise. There does not appear to be any order or rule which these low pressures follow. The only thing remarkable is that four of them in succession occured in the month of January, and with one exception they all took place in the morning.

The directions of the wind during the low atmospheric pressures indicated in the above table were as follows:—

1865. January 14th, S.S.E. through S. to W.
1867. January 8th, S.E. persistent.
1872. January 24th, N. through W. and S. to E.S.E.
1873. January 19th, S.S.W. veering alternately to S.S.E. and N.W.
1876. March 10th, S.S.W. veering alternately to S. and through W. and N. to N.N.E.
1876. December 4th S.E. veering alternately through E. to N.E. and through S.W. and N. to N.N.E.

Mr. **Baxendell** read the following letter from Mr. **Joseph Sidebotham**, F.R.A.S., dated Mentone, December 7th, 1876:—

There is a little matter I wish you would call attention to at one of the meetings of the Lit. and Phil. Soc., in order that the remarks of the members upon it may be obtained, and if thought desirable, published in the "Proceedings," so as to come before the public.

My attention has been for some time directed to the growing use of the anneline colors for tinting photographs. Now I find they are being *extensively* used in paintings and water-color drawings, and the colors regularly sold for that purpose. Any one who knows the speedy alteration by light of nearly all of these colors will protest against their use, and a statement of this with the authority of some of our chemists would probably have the effect of causing them to be discontinued by all artists who care to think that their works should last more than a single year.

Professor C. **Schorlemmer**, F.R.S., exhibited the new colouring matters *Fluorescein, soluble Eosin,* and *insoluble Eosin,* as well as samples of silk and wool dyed by them, and showed the characteristic properties of these bodies.

"On a Mineral Water from Humphrey Head, near Grange-over-Sands, North Lancashire," by JOSEPH BARNES and HARRY GRIMSHAW, F.C.S.

The mineral spring of which the following analyses have been made issues from the western side of the low promontory of Humphrey Head, almost at the base of the cliffs, and near to the sea shore, but quite above the ordinary high-water mark. The base of the rocks of which the substratum of the head is composed is washed by the tide when high, though it is far away from the water at its ebb in the wide and flat Morecambe Bay, in which the Humphrey Head forms the division between the Milnthorpe, or Lancaster Sands, and the Ulverston, or Leven Sands. The north and east sides of the head descend in a gentle slope to the sands, whereas the west and south sides present a rugged and perpendicular face of limestone rock.

As regards the geology of the Head; the strata of which it is composed, consist of mountain limestone, based upon the upper Silurian, which structure, we learned from Professor Dawkins, is common to a great portion of that district. Humphrey Head therefore seems to form a small outlying spur of the mountain system of the district, but it is completely separated by a strip of low-lying land from the nearest connected spur of the high lands of the neighbourhood, which is Kirk Head and contains the well known cave in which such interesting antiquarian discoveries have been made. The whole of this neighbourhood is remarkable for the general absence of streams or springs of any description, owing doubtless to the very porous nature of its rock formations, which allow the water to percolate down to the sea-level instead of finding their way along the surface to any extent.

The mineral spring which issues as described near the base of the Humphrey Head, was formerly and we suppose is still known by the name of the Holy Well. Two samples of its water have been taken, the first (*a*) on August 29th, 1875, and the second (*b*) on the 20th of the same month 1876. The following data were obtained on analysis.

Sample (*a*) had an almost imperceptible Alkaline reaction.

(1) Specific Gravity—Bottle holds 25·031 grams Water.
„ „ „ 25·176 „ Mineral Water.
Specific Gravity—1·00579 at 15° C.

(2) Ammonia—Half a litre distilled by Wanklyn's process gave: 0·47 grams free Ammonia and ·03 of Alluminoid Ammonia, equal to 0·94 and 0·06 pts. per million respectively.

(3) Chlorine—10 c.c. of the water required 33· 1 c.c. Standard Nitrate of Silver (1 c.c.=1 m. grm. Cl).

(4) Total Solid Matter—100 c.c. on evaporation gave 0·729 grm. which on analysis gave—

(5) Silica—0·0006 grm.

(6) $BaSO_4$—0·294 containing 0·1008 SO_3.

(7) $CaCO_3$—0·1088 „ 0·0608 CaO.

(8) $Mg_2P_2O_7$—0·08 „ 0.0288 MgO.

(9) Traces of Iron and Alumina were found.

Sample (*b*) had a neutral reaction.

(1) Total Solid Matter—As a mean of three determinations 100 c.c. gave 0·7352 grams.

(2) Chlorine—As a mean of two determinations 5 c.c. took 16·65 c.c. Stand. Ag. NO_3.

(3) Hardness—10 c.c. were made up to 250 c.c. and 70 c.c. gave 4·1° of Hardness.

(4) Iron and Alumina—Traces were found.

Sample (*a*).	Grains per Gal.	Sample (*b*) Grains per Gal.
Silica (SiO_2)	0·42	
Sulphuric Acid (SO_3)	70·50	
Lime (CaO)	42·50	
Magnesia (MgO)	4·89	
Magnesium (Mg)	9·37	
Chlorine (Cl)	231·70	233·1
Alkalis (K and Na)	150·92	
Total Solids	510·30	514·64
Hardness		102·50

A calculation of these results on the usual assumptions of the combination of the different bases and acids, yields the following result for sample (*a*), leaving the small amounts of Silica and Ammonia out of consideration.

Silica.....................	0·42	Grms.	per Gal.
Calcium Sulphate......	103·21	"	"
Magnesium Sulphate...	14·68	"	"
Magnesium Chloride...	37·07	"	"
Sodium Chloride	270·03	"	"
Potassium Chloride ...	84·90	"	"
	510·31		

The lime is placed as in combination with Sulphuric Acid, as forming the most insoluble compound with it, and that a considerable amount of the Magnesium is combined with Chlorine is evident from the fact, that an estimation of Chlorine in the solid residue, after heating it for some time to 170° C. gave only 219·8 grains of Cl. per gallon, showing a loss of 11·9 grains. The relative amounts of Potassium and Sodium Chlorides are calculated by difference, from the total Alkali and Chlorine.

Remarks on results of Analysis.—The proportion between the free and Alluminoid Ammonia, in this water is very remarkable, the amount of the former being conspicuously large (0·94 pts. per mil), whereas the latter is hardly more than detectable (0·06). The amounts of Alkaline Chlorides (354·93 grms. per gal.) is very large, mostly consisting as will be seen of Chloride of Sodium. We are of opinion that this is due to the upward absorption of a certain amount of sea water by the porous rocks through which the waters of the spring find their way, in fact the whole contents of the water seem to be only such as might be naturally expected from the nature of the rock construction of the Humphrey Head and its proximity to the sea. It is stated in works referring to this district that this spring was at one time in high repute for the medicinal qualities of its water, more especially in the cure of various cutaneous disorders, but at the present time it is seldom visited except from curiosity, though a few of the country people around seem to have a lingering belief in its virtues.

For some of the figures in the above analyses we owe our thanks to Mr. Chaster, late a student of the Owens College.

Since the above was written, our attention has been directed to an analysis of the water of this spring, published in the year 1868 in the Journal of the Chemical Society (2nd Series VI 20), by T. E. Thorpe, (now F.R.S.), in which the Author gives more minute details of the constituents of the water, than we have done in this paper, but which in the main shows that the quality of the water remains very constant. The exception to this is in the amount of Potash, which in our case (calculated from the amounts of acid and base) is very mnch larger than in Thorpe's analysis. We hope at a later date to perform other and more minute analyses of this spring, in order to see as far as possible whether this uniformity is still maintained.

"On Ternary Differential Equations," by Sir **James Cockle**, F.R.S., Corresponding Member of the Society.

1. Suppose that the criterion of integrability is not satisfied for

$$dz = \mathrm{M}dx + \mathrm{N}dy \;\dots\dots\dots\dots\dots\dots\dots\dots \quad (1)$$

and is satisfied for

$$dz = pdx + qdy \;\dots\dots\dots\dots\dots\dots\dots\dots \quad (2)$$

which is the same thing as supposing that when

$$\frac{a\mathrm{M}}{dy} + \mathrm{N}\frac{d\mathrm{M}}{dz} - \frac{d\mathrm{N}}{dx} - \mathrm{M}\frac{d\mathrm{N}}{dz} = \mathrm{U} \text{ and } \frac{dp}{dy} + q\frac{dp}{dz} - \frac{dq}{dx} - p\frac{dq}{dz} = u.$$

then U is finite and u is identically null.

2. Using D to denote total and d partial differentiation, we may write the last two equations in the respective forms

$$\frac{DM}{dy} - \frac{DN}{dx} = U \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

$$\frac{Dp}{dy} - \frac{Dq}{dx} = u \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4)$$

and if we put

$$M = P + p \text{ and } N = Q + q$$

then, developing and remembering (4) and putting $u=0$,

$$\frac{DP}{dy} - \frac{DQ}{dx} = U \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (5)$$

3. Let $\phi\ (x, y, z, c)=0$ be the complete solution of (2) and suppose that some particular solution, say $\phi\ (x, y, z, a)=0$ is also a solution of (1). Then $\phi\ (x, y, z, a)=0$ gives $M=p$ and $N=q$, and therefore

$$P = 0,\ \frac{DP}{dx} = 0 \text{ and } \frac{DP}{dy} = 0$$

$$Q = 0,\ \frac{DQ}{dx} = 0 \text{ and } \frac{DQ}{dy} = 0$$

and consequently, by (5), it gives $U=0$. Hence if (1), not being integrable, has a single solution, that solution is contained in $U=0$.*

4. If we develope and then eliminate p between $\frac{DP}{dx} = 0$ and $\frac{DQ}{dx} = 0$ and q between $\frac{DP}{dy} = 0$ and $\frac{DQ}{dy} = 0$, we find

$$\frac{dP}{dx}\frac{dQ}{dz} - \frac{dQ}{dx}\frac{dP}{dz} = 0 \text{ and } \frac{dP}{dy}\frac{dQ}{dz} - \frac{dQ}{dy}\frac{dP}{dz} = 0$$

which, combined, give

$$\frac{dP}{dx}\frac{dQ}{dy} - \frac{dQ}{dx}\frac{dP}{dy} = 0$$

Hence P and Q are functions one of the other.

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* I call such solutions "discriminoidal." For an example of this discriminoidal solution see Art. 26 of my paper "On Particular Integrals" in the Quarterly Journal of Pure and Applied Mathematics (vol. XIII. p. 239). In the present communication to the Society it will be seen that I do not have any recourse whatever to the general dual solution.

"Oakwal" near Brisbane, Queensland, Australia.

October 10th, 1876.

5. If we develope

$$\frac{d}{dz}\left(\frac{\mathrm{D}p}{dy}-\frac{\mathrm{D}q}{dx}\right)=\frac{\mathrm{D}}{dy}\frac{dp}{dz}-\frac{\mathrm{D}}{dx}\frac{dq}{dz} \quad \ldots\ldots\ldots\ldots\ldots \quad (6)$$

we shall see that it is an identity; for the sinister only differs from the dexter in having the terms

$$\frac{dp}{dz}\frac{dq}{dz}-\frac{dp}{dz}\frac{dq}{dz} \text{ which}=0,$$

or cancel one another. Hence

$$\frac{dp}{dz}dx+\frac{dq}{dz}dy$$

is a perfect differential, not in general integrable in its present form. It is in fact the differential of the logarithm of the reciprocal of the integrating factor of (2).

ERRATA.

Page 34, for lines 8, 9, 10, 11, 12, 13 read—"I can merely bring before you a few of the more noteworthy points which stand out from my general reading, more especially relating to the History of Ancient Life on the earth, together with some general considerations which seem to me opportune under the circumstances of the time."

Page 40, line 4 from the bottom, for "fluviable" read "fluviatile."

Page 40, last line, for "the equivalent of" read "as old as."

Ordinary Meeting, December 26th, 1876.

E. W. BINNEY, F.R.S., F.G.S., President, in the chair.

Notice of the "Almanacke for XII Yere," printed by Wynkyn de Worde in 1508, by WILLIAM E. A. AXON, M.R.S.L., &c.

Some members of the society may be interested in this curious work, of which a transcript has recently been made for me. The "Almanacke for XII yere" is of excessive rarity. There are only two copies known to exist. One was found by Dr. Bandinel in the Bodleian Library, and the other is in the British Museum. It is one of the smallest books in the world, measuring only two inches and a half by two inches.

The object of the undertaking is thus set forth :—

"This almanacke and table shall endure XII yere and is calked after the latytude of Oxēforde / and it is taken out of the grete ephymerides or almanacke of XXX yere / and sheweth the coniunccios that is to say the metynge & fyrst lyghtnynge that the mone taketh from the sonne the whiche is called the chaunge or the newe mone amonge us. And the opposycyons that is to say the full mone whan we se it full & roūde * * * Also ye shal fynde every yere how longe the flesshe tyme is betwene Crystmas and lente / & that is called Intervallū / and there ye shal see how mani weekes and dayes the tyme is betwene Crystemas and lente & so forthe shal ye fynde Septuagesima that is whan Alleluya Gloria i excelsis / & Te Deum is layde down in holy chyrche / and than foloweth Quadragesima that is the fyrst sondaye in clene lent and then ye shall fynde eester day, Rogacion daye, Ascencyon day Whytsonday / and Advent sonday. And also ye shal fynde the eclypses betwene the sonne and the mone with the daye / hour / and mynute folowynge / lately corrected and emprynted at London in the Fletestrete by wynkyn de worde. In the yere of the Incarnacyon of our lorde A.M. CCCCC and viii. The xxiii yere of the reygne of our most redoubted souverayne lord kinge Henry the VII."

In the tables which follow it is noticeable that whilst only Roman numerals are employed throughout, the cipher is used to show that some places in the column have not been left blank by accident. Whether this is taken from the Arabic numerals, or is a survival of the *sipos* used in the abacus named after Boethius might be difficult to determine.

"A Notice of some Organic Remains from the Manx Schists," by E. W. BINNEY, F.R.S., F.G.S., President.

[This Paper will appear in a succeeding number of the Proceedings.]

"On Changes in the Rates of Mortality from Different Diseases during the Twenty Years 1854-73," by JOSEPH BAXENDELL, F.R.A.S.

It is well known to all who take an interest in questions relating to health and disease that the sanitary measures which have been carried out, at great cost, in all parts of the country during the last twenty or thirty years appear to have had no perceptible effect in improving the general state of the public health and reducing the rate of mortality, and a feeling is rapidly gaining ground that the sanitary schemes which have been imposed upon the country by parliament are either of very questionable utility, or have been carried out by local authorities in a very negligent and inefficient manner, while the enormous and rapidly increasing expenditure which they involve is becoming a matter for very serious consideration, and is especially annoying and disappointing when it is seen that they have so far signally failed to produce the results which their promoters so confidently anticipated. Occasionaly when a season or succession of seasons favourable to the public health has reduced the death rate for a time, sanguine officials have hastily and inconsiderately attributed the reduction to their so-called sanitary improvements and arrangements; but when a period is taken sufficiently long to eliminate the influence of changes in the seasons it is found that no reduction whatever has taken place, but that on the contrary the rate has still a slight tendency to increase. Thus the average death rate for all England and Wales during the ten years 1854-63 was 22·14, while in the following ten years 1864-73 it was 22·45, or 1·4 per cent greater.

That the present sanitary system is very defective and not likely to produce any material improvement in the public health is virtually admitted by many officers of health, who now frequently state in their reports that the deaths which have occurred in their districts were from causes over which the board or sanitary authority had no control, by which it is commonly understood that the diseases which caused the deaths were not contagious or infectious, or as they are now frequently termed, "preventible," the operation of the present system being thus confessedly limited to a group of diseases which cause only about one seventh of the total number of deaths. But we have seen that during the last twenty years there has been no diminution in the general death rate, and it is evident therefore either that sanitary measures have failed entirely to reduce the mortality from the so-called preventible diseases, or that any reduction they have effected has been counterbalanced by a corresponding increase in the mortality from the non-preventible class. With a view to determine which of these conclusions is the true one I have compared the rates of mortality from different diseases during the ten years 1854-63, with those for the succeeding ten years 1864-73, the year 1873 being the last for which an annual report has been published by the Registrar General.

The mean population of England and Wales for the first period was 19,592,113, and for the second 22,099,664, the ratio of these numbers being as 1 : 1·1280.

The following table shows the number of deaths in each period from each of eight infectious diseases; the corresponding ratios; the differences between these ratios and that of the increase of population; and the percentages of increase or decrease of the death-rates:

	Total Deaths.		Ratios.	Differences from Ratio of Increase of Population.	Percentages of Increase or Decrease of Death-rate.
	10 Years, 1854–63.	10 Years, 1864–73.			
Scarlet Fever and Diphtheria	218,248	227,849	1·0439	—·0841	—7·4
Fever, Typhus, Typhoid, and simple continued Fever...	169,511	180,467	1·0646	—·0644	—5·6
Whooping Cough	104,353	110,722	1·0610	—·0670	—5·9
Measles....................	88,254	89,121	1·0098	—·1182	—10·4
Croup	52,160	47,562	0·9118	—·2162	—19·1
Smallpox	33,515	70,458	2·1023	+·9743	+86·3
Erysipelas	18,510	19,320	1·0389	—·0891	—7·9
Influenza..................	13,667	5,174	0·3859	—·7495	—66·4

It appears therefore that of these eight infectious diseases the mortality from seven has diminished very sensibly, while that from the eighth, small-pox, shows an extraordinary increase. But this disease is the only one for which we have a special preventive, and yet although this preventive has been almost universally applied under the powers of Acts of Parliament, and all the other infectious diseases appear to have yielded in various degrees to the influence of ordinary sanitary improvements, this disease has increased at a rate greatly exceeding that at which any other infectious disease has decreased, and even, as I shall presently show, greatly exceeding that at which any other disease whatever has increased or decreased, thus strongly suggesting the idea that special means must have been taken to encourage its development.

Thirty years ago it was commonly supposed that small-pox occurred much more frequently among children and young persons than among adults, and I find that in the five years, 1848-52, the first for which we have accurate returns, the deaths of males under 5 years of age were 66·5 per cent. of the total number of deaths at all ages, but in the 5 years 1869-73 the proportion had fallen to 31·0 per cent. For females the numbers were respectively 72·2 and 36·1 per cent. The proportion of deaths at all ages above 5 years was therefore more than doubled. But taking the deaths between the ages of 25 and 65 the proportion was more than trebled in the case of males, and more than quadrupled in the case of females; and taking into account the increase in the total mortality at all ages it appears that the death-rate of males between the ages of 25 and 65 years was 4·46 times greater in 1869-73 than in 1848-52, and of females 5·14 times greater. These remarkable facts seem to indicate that this loathsome disease has in some way become very much modified, and more virulent, or that some cause has been in operation which has rendered the adult portion of the population very much more susceptible to its attacks, and it is evident therefore that it is now a much more serious evil to society than it was 20 or 30 years ago, not only because the mortality from it has greatly increased, but also because a very much larger proportion of that mortality now falls upon the producing classes. In spite of sanitary improvements, and the protection supposed to be afforded by the application of a special preventive, the death-rate from it has increased at all ages without exception, but more especially at those ages when it is most important for the general interests of society that life should be saved.

The general conditions which tend to produce an outbreak of small-pox also favour the development of scarlet fever, so that if a marked increase takes place in one, an equally marked increase may be soon expected to appear in the other; and yet, while scarlet fever has been favourably influenced by ordinary means and treatment, small-pox has not received the slightest check, but, on the contrary, has gone on increasing in fatality in spite of the application of both ordinary and extraordinary means for its suppression.*

The results given above seem to indicate clearly that the infectious diseases, as a class, have, so far, been favourably acted upon by the carrying out of sanitary measures, and the mortality from them sensibly reduced. It follows therefore that the total mortality from diseases of the non-preventible class must have increased to a slightly greater extent, and it becomes a matter of considerable interest and importance to ascertain which of these diseases have contributed to this increase, and in what proportions.

An examination of the returns soon showed that considerable changes had taken place in the mortality from several of the diseases which are usually regarded as not being under the control of sanitary officials, and that while in some it had increased, in others it had diminished, though on the whole to a somewhat less extent.

The principal non-preventible diseases which increased in fatality, and the rates of increase were as follows:—

	Per cent.		Per cent.
Bronchitis	35·1	Tabes Mesenterica	16·1
Heart Disease	32·3	Cephalitis	15·0
Diarrhœa	26·3	Rheumatism	13·5
Lung Disease	24·0	Brain Disease	11·4
Cancer	22·0	Apoplexy	11·1
Liver Disease	18·1	Premature Birth	9·0
Kidney „	16·7	Paralysis	8·2

The following are the principal non-preventible diseases which have diminished in fatality, and their rates of diminution:—

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* Since this was written the Registrar General's Annual Report for 1874 has been issued, and I find that the deaths from scarlet fever rose from 13,144 in 1873, to 24,922 in 1874. This was a clear indication that another outbreak of small pox was at hand, and accordingly, in the following year, 1875, the epidemic commenced from which we are now uffering.

	Per cent.		Per cent.
Dropsy	31·4	Hydrocephalus	11·3
Asthma	25·2	Old Age	8·1
Enteritis	19·6	Convulsions	7·5
Scrofula	17·2	Phthisis	7·1
Pneumonia	16·6		

It has not been thought worth while, for the present at least, to include in these lists diseases which have increased or decreased in fatality less than 7 per cent., or from which the number of deaths averaged less than 2,500 per annum during the last ten years.

An analysis of the death returns of a few of the more important of the diseases in the above lists has yielded the following results.

Bronchitis.

During the ten years 1854-63 the total number of deaths caused by this disease was 277,335, and in the following ten years it rose to 422,806. We have then

$$\frac{\left(\frac{422806}{277335}-1{\cdot}1280\right)100}{1{\cdot}1280}=+35{\cdot}1.$$

The increase of the death-rate from bronchitis therefore amounted to 35·1 per cent., or to a loss of no less than 109,972 additional lives, while the total number of lives saved by the improvement in the whole class of infectious diseases amounted to only 36,995, or one-third of the extra loss from bronchitis alone, so that while all the sanitary improvements of the last twenty years may be regarded as having effected a saving of 1 life in every 134, the increased mortality from bronchitis alone has resulted in a loss of 1 in every 45. This great increase in the death-rate from bronchitis is the more remarkable from the fact that it has been gradual and steady, and appears to have been but little influenced by changes in the character of the seasons. Thus the mean death-rate during the five years

1854-58 was 1·296
1859-63 „ 1·528
1864-68 „ 1·778
1869-73 „ 2·040

Comparing the rate during the last five years with that during the first five the increase is 57·4 per cent!

We have seen that the increase in the mortality from small-pox was relatively much less among infants and young children than among adults; but with bronchitis the reverse of this took place. Thus in the five years 1854-58 the deaths of males under 1 year of age were 21·2 per cent of the total deaths at all ages, and under 5 years 36·3; but in the five years 1869-73 the numbers were respectively 25·9 and 41·2. It is remarkable however that between the ages of 10 and 25 there was a slight decrease, so that the curve representing the variations in the mortality at different ages from bronchitis has two well marked maxima—one at ages under 1 year, and the other between the ages of 55 and 65. The increase at ages under 1 year amounted to 49·7 per cent, and at ages between 55 and 65 it was 35·6 per cent. During the year 1873, out of every 19 deaths from all causes, two were due to bronchitis alone. In the 7 years 1848-54 the proportion was 2 deaths from bronchitis out of every 47 from all causes.

I have already remarked that the increased mortality from bronchitis cannot be attributed to changes in the character of the seasons. It is however due to some cause which affects the entire population of the kingdom, both male and female, since I find that the increase has been general, and not confined to smoky towns and manufacturing districts, as will be seen from the following comparison of the death-rates in North Wales and three of the healthiest counties in England, Westmorland in the north, and Sussex and Dorset in the south, and in Lancashire, the most unhealthy county.

	Mean Death-rate, 1854—63.	Mean Death-rate. 1864—73.	Increase per Cent.	Percentage of Total Death-rate, 1854—63.	Percentage of Total Death-rate, 1864—73.
North Wales	0·690	1·092	58·2	3·32	5·13
Westmorland	0·845	1·265	49·7	4·66	7·18
Sussex	0·945	1·269	34·2	4·96	6·85
Dorset	1·029	1·468	42·6	5·41	8·01
Lancashire	2·203	3·000	36·1	8·48	10·98

Heart Disease.

This disease ranks next in order after bronchitis in the rate at which its fatality has increased. In the first decade the total number of deaths was 155,850, and in the second, 232,566. Allowing for the increase of population, these numbers show an increase of no less than 32·8 per cent in the death-rate. Sixty per cent of the deaths occurred between the ages of 45 and 75, the maximum rate being between the ages of 65 and 75; but the greatest increase of the rate took place between the ages of 25 and 35, and amounted to 41·5 per cent.

Diarrhœa.

The total number of deaths from this disease in the first ten years was 154,513, and in the second, 220,173. These numbers give an increase of 26·3 per cent in the death-rate. For males the increase was 27·5 per cent, and for females, 25·0 per cent. The death-rate of males under one year of age increased 42·5 per cent, and all under 5 years 36·0 per cent; but at five years and all above it diminished 7·1 per cent.

Cancer.

The deaths from cancer during the first 10 years were 65,989, and in the second 10 years 90,808, giving an increase of 22·0 per cent in the death-rate. For males the increase in the rate was 25·5 per cent, and for females 20·4 per cent. The maximum rate of increase occurred with males between the ages of 45 and 55, and amounted to 31·5 per cent; and with females between the ages of 55 and 65, the amount being 23·3 per cent. For every 100 deaths of males there were 230·4 deaths of females in the first 10 years, and 220·8 in the second 10 years.

Cephalitis.

This disease merits attention as being one which is gradually becoming more fatal to infants and young children.

In the first 10 years it was the cause of 35,331 deaths, and in the second 10 years of 45,827, the increase in the rate or mortality being 15·0 per cent; but with male infants under 1 year of age it amounted to 32·5 per cent; and for infants and young children under 5 years it was 26·1 per cent, while for males at all ages above 15 there was a decrease of 11·8 per cent.

The gradual and steady change which has taken place during the last 25 years in the incidence of mortality from cephalitis upon different ages will be seen from the following numbers :—In the 7 years 1848-54 the number of deaths under 1 year was 14·6 per cent of the total number of deaths at all ages; in the following 7 years it was 16·8 per cent; in the next 6 years 18·3 per cent; and lastly, in the 6 years 1868-73 it was 20-1 per cent. The percentages of deaths at all ages under 5 years for these periods were 42·6; 46·7; 49·4; and 52·8.

The total number of deaths in each ten-yearly period from the remaining non-preventible diseases which have increased in fatality were as follows: —

	10 Years, 1854—63.	10 Years, 1864—73.
Lung Disease	35,390	49,503
Liver Disease	42,778	57,001
Kidney Disease	21,186	27,910
Tabes Mesenterica	50,983	66,789
Rheumatism	19,840	25,415
Brain Disease	43,727	54,979
Apoplexy	87,750	110,011
Premature Birth	74,670	91,851
Paralysis	91,697	111,994
	468,031	605,453

The average increase from all these diseases is 14·6 per cent, which represents an extra loss of more than 77,000 lives, or more than double the number saved by the improvement in the infectious diseases.

Among the diseases which have diminished in fatality, more than 80 per cent of the total number of deaths were caused by phthisis, old age, convulsions, and pneumonia The diminution in the number of deaths from old age will perhaps be regarded as an unfavourable result which is hardly compensated by the diminished fatality among infants and young children from convulsions.

The deaths from old age and convulsions in the 10 yearly periods were:—

	10 Years 1854-63.		10 Years 1864-73.
Old age.........	272,434	......	282,490
Convulsions...	251,330	......	262,170

Phthisis.

The disease which caused the greatest number of deaths in both the ten-yearly periods was phthisis. In the first ten years the number of deaths was 508,210, and in the second 532,780; but allowing for the increase of population there was a decrease of 7·1 per cent in the rate of mortality. This decrease however was not equally shared by the two sexes, the decline of the rate among males being only 2·4 per cent, while among females it amounted to 12·6 per cent; nor was there a decrease at all ages, but on the contrary between the ages of 25 and 65 there was an increase of 4·5 per cent. More than half the deaths from phthisis occurred at ages under 35 years; and more than 45 per cent between the ages of 15 and 35.

Pneumonia.

The deaths from pneumonia in the first ten years were 242,757, and in the second 228,069, the decrease in the death-rate being 16·6 per cent. With males the decrease was 15·6 per cent, and with females 18·8 per cent. There was however, as in the case of phthisis, an increase between the ages of 25 and 65, the amount being 13·7 per cent. In the first ten years the deaths under 5 years of age were 68·1 per cent of the deaths at all ages; but in the second ten years the proportion was reduced to 60·8 per cent.

The deaths from dropsy, asthma, enteritis, scrofula, and hydrocephalus in the two ten-yearly periods were as follows: —

	10 Years, 1854—63.	10 Years, 1864—73.
Dropsy	82,085	63,485
Asthma	42,907	36,173
Enteritis	32,842	29,781
Scrofula	30,219	28,223
Hydrocephalus	73,620	73,652
	261,673	231,314

The mean decrease in the rate of mortality from these diseases was 21·6 per cent. which represents a saving of 63,886 lives.

There is one disease which owing to the deaths caused by it not averaging 2,500 per annum during the last ten years has not been included in the list of those which have increased in fatality, but which I think ought not to pass without notice. This is Nephria or Brights' disease. In the ten years 1854-63 the deaths from it were 11,948; but in the following ten years they were 23,802. The increase in the death-rate from this cause therefore amounted to 76·6 per cent., or more than double the rate of increase of bronchitis.

It is very commonly supposed that all the zymotic diseases are preventible, or under the control of sanitary authorities, and it may be objected that I have taken only the eight infectious diseases to test the utility of the sanitary measures which have been carried out during the last 20 or 30 years. To meet this objection it may be well to state that the total number of deaths from all the zymotic diseases in the ten years 1854-63 was 973,248, and in the following ten years, 1,109,824. These numbers give an increase of 1·08 per cent., and therefore if sanitary measures are to be tested by the impression they make upon the death-rate from the whole class of zymotic diseases it is evident that the measures hitherto adopted have entirely failed to produce any improvement of the public health.

The results which I have now laid before the Society, though perhaps not so full and complete as might be desired are, it seems to me, quite sufficient to justify the following conclusions: —

1.—That sanitary measures, tested by their effect on the diseases which are universally admitted to be infectious, have produced a slight improvement in the public health which is equivalent to a saving of 1 life in every 134, or a reduction of 0·17 in the general death-rate; but tested by their effect on the whole class of zymotic diseases they have caused no improvement whatever, and have not even prevented an increase in the mortality from these diseases which is equivalent to an extra loss of life in every 417.

2.—Considering the great increase in the fatality of smallpox when that from all other infectious diseases has diminished, that it has now become relatively much more prevalent among adults than at any time since vaccination was introduced; that the experience of the last and of the present epidemic has furnished abundant proof that vaccination does not now, as it did in Jenner's time, and for many years after, afford an almost certain protection against its attacks; and that the question of the probability of its gradually becoming greatly modified and producing results that may be seriously unfavourable to the public health has, apparently, never been considered and investigated, it is desirable that an authoritative inquiry into the whole subject should be at once set on foot in order to ascertain, if possible, why vaccination has now become so much less efficacious as a preventive of smallpox than it was for many years after its general adoption; what has been the true cause of the great increase ot smallpox during the last 15 to 20 years; and why adults have become so much more liable to its attacks?

3.—That the reduction in the rate of mortality from the class of infectious or preventible diseases has been more than counterbalanced by an increase in the rate from the non-preventible class; and that owing to the changes which have taken place in the latter, the mortality from diseases which run their course rapidly, and cause comparatively sudden deaths, has greatly increased.

Ordinary Meeting, January 9th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that he had received from Mr. B. H. Green, of this city, more papers of the late Mr. George Walker, one of the original members of this Society, relating to the early importations of cotton into England.

99,000	B. & P. West India, supposed 160lbs. each.
13,400	Bales Turkey, „ 260lbs. „
112,400	Packages of Cotton imported into Great Britain during the year 1790.

Of which it is supposed the Manufactories have used:

		lbs.		
80,000	W. India at 160lbs. nett each,	12,800,000	@ 15½d.	= £827,000
12,000	Turkey at 260lbs. „	3,120,000	@ 10½d.	= £136,500
92,000	B. & P. used.	15,920,000		= £963,500

15,920,000 lbs. @ 6s. 8d. = £5,310,000—Sterling when made into Cloth and finished for sale.

£5,310,000	
963,500	
£4,346,500	Wages and profit.

The profit realised by the manufacture in those days appears to have been very considerable, and no cotton is described as coming from the United States.

Another table of the importations into Liverpool in the year 1812 and the six preceding years Mr. Walker gives as from Mr. Aspinwall, and is as follows.

MONTH.	America.	New Orleans.	Brazils and Portugal.	Demerara and Berbice.	Surinam.	Trinidad.	Barbadoes.	St. Domingo.	Dominica and Grenada.	Tortola.	Antigua.	St. Vincent, Nevis, &c.	St. Thomas.	Jamaica.	Bahamas.	St. Lucia.	Ireland.	Mediterranean Azores, &c.	St. Croix.	Havannah.	TOTAL.	Monthly Sales.
January	2917	...	2180	12	..	..	..	..	..	..	..	..	..	..	102	..	370	..	...	...	5581	18875
February	8751	1141	1388	945	..	..	281	..	..	..	..	..	..	..	..	..	388	310	..	..	13204	15918
March............	4058	543	365	497	..	..	..	..	..	..	..	..	..	..	..	..	50	..	..	..	5515	7192
April	13734	784	6413	2330	..	..	41	..	..	..	..	..	..	..	..	..	152	15	..	..	23460	12537
May	30232	6270	4359	6359	34	..	155	..	..	153	..	..	..	766	210	9	30	..	..	..	48603	25839
June	1452	6559	11906	2423	265	2	282	..	..	..	..	60	50	682	9	..	491	..	50	..	24231	15462
July	278	1524	13375	1803	200	..	587	..	58	..	..	42	..	226	358	7	..	..	..	..	18458	28142
August	..	..	4575	2260	..	..	362	..	199	..	21	59	..	178	..	12	..	537	..	..	8203	20507
September	..	..	4345	448	..	110	206	..	..	243	4	..	..	324	..	..	308	..	..	..	5988	26753
October	800	..	8018	1002	.	..	459	..	..	..	..	..	..	28	326	..	105	146	..	..	10963	20113
November	90	...	2285	..	.	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2375	17662
December	290	..	2690	482	42	17	523	..	..	80	..	..	610	..	..	13	58	..	..	..	4805	43432
Total of 1812......	62701	16827	61899	18561	541	129	2896	..	257	476	25	161	660	2204	1005	41	1972	1008	50	..	171413	252432
Ditto of 1811......	72653	24749	43698	19753	734	514	2869	..	..	119	15	217	361	987	1309	78	1868	342	32	290	170088	198609
Ditto of 1810......	185742	12094	75328	22237	1086	695	4163	114	231	193	640	790	66	6307	3730	144	5074	1340	358	356	320688	..
Ditto of 1809......	120359	6919	85785	15158	837	618	6331	105	364	529	349	1113	191	8202	2023	393	4153	5650	3187	5080	267346	..
Ditto of 1808......	19801	5613	6640	15298	1051	662	5229	726	308	891	263	360	2262	3069	1126	622	2304	491	7	..	66203	..
Ditto of 1807......	116281	27585	11852	21092	1331	608	4825	24	1525	1102	324	281	2422	4170	1654	1121	359	..	..	..	196556	..
Ditte of 1806......	87659	13336	35628	17324	1130	287	5495	..	1375	1325	278	490	1743	4011	1980	1399	546	..	..	..	174015	..
TOTAL OF 7 YEARS	665190	107123	320830	129423	6699	3513	31808	969	4060	4135	1894	3412	7705	28950	12827	3798	15776	8831	3634	5726	1366309	..

The total imports into Liverpool for seven years is 1,366,309 packages, making an average of 195,184 packages per year; or 3,753 packages per week. The total imports into the Kingdom for the last four years runs, for 1809, 434,560 packages; 1810, 56,090; for 1811, 321,250; and for 1812, 258,620 packages. Total, 1,574,520, averaging 393,630 packages per year, or 7,570 packages per week.

April 24th. The ship Tobin arrived at Liverpool from Bahia with 4,296 bags of cotton.

From this table it appears that from the years 1806 to 1812 American cotton had been imported in large quantities.

"On the Poisonous Properties of Yew-leaves," by **JAMES BOTTOMLEY**, D.Sc.

In the *Field* for December 23rd is a letter from Professor R. V. Tuson, relative to the poisonous action of yew-leaves on pheasants. At the end of his letter he promises to make an examination of the toxic properties of this tree, which up to the present time do not seem to have been fairly investigated. In November, 1871, a similar case came under my notice. A number of pheasants were found dead on the estate of Mr. Baring, in Hampshire. Four of the birds were sent to me for examination. Three of them contained in their craws considerable quantities of yew-leaves along with grain. The craw of the fourth bird contained neither grain nor yew; there was only a little mucus, distended with bubbles of air. The yew had no doubt passed into the alimentary canal and could not be very distinctly recognised. In the course of the investigation I found that apart from botanical characteristics the yew could yield well marked chemical reactions. It became necessary to subject yew-leaves to the same process as would be used to separate strychnine from animal tissue, the ethereal extract finally obtained having been evaporated to drive off the ether. There remained on the evaporating basin a thin varnish-like residue which was very bitter to the taste. When treated with cold nitric acid this residue assumed a dark blue colour like indigo. Upon the application of heat the colour disappeared.

"On the Luminous Sulphides of M. Ed. Becquerel," by WILLIAM THOMSON, F.R.S.E.

My object in bringing this communication before the society is to shew what I consider to be some most interesting substances, viz., the Sulphides, principally of the Alkaline Earths, the luminous properties of which were studied many years ago by M. Edmond Becquerel and others. These samples which I have I received some time ago when in Paris through my friend M. Auguste Guerout, Prèparateur au Musèum d'Histoire Naturelle. They were prepared by M. André, the laboratory assistant of M. Ed. Becquerel, who has devoted much attention to the peculiarities of manipulation required to produce the greatest degree of luminosity in the sulphides after holding them for a few seconds before the sun's light or a piece of burning magnesium wire or other source of light and then placing them in the dark; doubtless many present will be familiar with the results arrived at by Becquerel and others, but a short résumé of some of them may not be out of place here.

When these sulphides, which must be kept in hermetically sealed tubes to prevent oxidation, are exposed to the rays at different parts of the spectrum, these rays have very different actions in rendering the sulphides luminous, and also in some cases of producing slightly different shades of colour. The visible part of the spectrum has little or no power to render these bodies phosphorescent, the violet part, however, has greatest action, and the ultra-violet rays produce the maximum effect. M. Becquerel found then, in these sulphides, an interesting method of examining the invisible parts of the spectrum, which furnished an addition to those employed, viz., the thermometer, silver compounds, etc.; these sulphides may be compared to chords which have the power of absorbing vibrations whose wave-lengths are too small to affect the organs of hearing—too shrill to be heard—and of changing them into vibrations of greater wave-lengths and emitting them, so that they may be distinctly heard.

When these sulphides are held before some source of light, the sun's rays for instance, and then placed in a dark room, they gradually, after many hours, lose their phosphorescence—if, however, they be then heated a few degrees above the temperature at which they have remained, say from 60° to 80° Fah., they again become faintly luminous, and if kept for some time at that temperature they gradually lose their phosphorescence again, if allowed then to cool in the dark to 60° Fah., and again heated to the same temperature they show no phosphorescence, but if heated to a still higher degree of temperature, say 100° Fah., they again become luminous, and so on; this power may be regained and lost any number of times by placing them for a few seconds before any bright source of light and then removing them to a dark room—an electric current from a Ruhmcoff's coil passed through vacuum tubes containing these sulphides also developes their different colours.

The interesting point with respect to the sulphides which I show here this evening, is that they have been prepared by a special mode of manipulation by M. André, and that the luminosity or phosphorescence capable of being produced by them is greater than that from any which hitherto have been prepared.

The following gives the compositions of the different sulphides which show the differently-coloured phosphorescences:—

Green is composed of sulphide of calcium.

Orange is composed of sulphide of calcium which has been heated with 1 or 2 per cent of binoxide of manganese.

The lime for the preparation of the above-mentioned two colours was produced by the calcination of oyster shells.

An orange-coloured phosphorescence is also produced by sulphide of barium.

Blue and *Violet* are each sulphides of calcium which have been prepared from precipitated carbonate of lime.

Yellowish Green is simply sulphide of strontium.

Yellow is sulphide of strontium which has been calcined with 4 or 5 per cent of sulphide of antimony.

"On the types of Compound Statement involving four Classes," by Professor W. K. CLIFFORD, M.A., F.R.S. Communicated by Professor W. S. JEVONS, M.A., F.R.S.

Professor Stanley Jevons has enumerated* the types of compound statement involving three classes, among which the premises of a syllogism appear as a type of four-fold statement. He propounded at the same time the corresponding problem of enumeration for four classes, which is solved in the present communication. The reader is referred to the paper or the book just mentioned for further explanation of the nature and purpose of the problem than is to be found in Art. 1. It may however be premised that the letters A, B, C, D, denote four *classes* or *terms* (for example hard, wet, black, nice), and that according to a convenient notation of De Morgan's, the small letters, *a*, *b*, *c*, *d* denote the complementary classes or contrary terms (not hard, not wet, not black, not nice). A *simple* statement is of the form ABCD$=$0. (no hard, wet, black, nice things exist, or, which is the same thing, all hard, wet, black things are nasty). The statement ABC$=$0 (no hard, wet, black things exist, or all hard, black things are dry) is to be regarded as made up of these two ABCD=0, ABC*d*=0 (no hard, wet, black, nice things exist and no hard, wet, black, nasty things exist) and so is called a *compound* (in this case a *two-fold*) statement. The notion of *types* is defined in Art 1.

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* Proceedings of the Manchester Phil. Soc. vol. vi. pp. 65—68, and Memoirs, Third Series, vol. v. pp. 119—130.

The Principles of Science, vol. i. pp. 154—164.

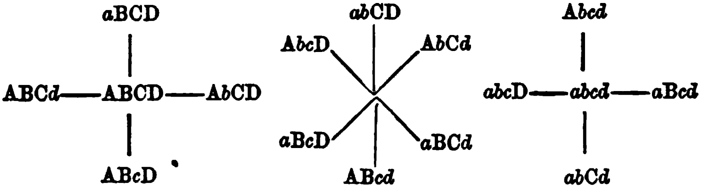
1. Four classes, or terms, A,B,C,D, give rise to sixteen cross-divisions or *marks*, such as A*b*C*d*. A denial of the existence of one of these cross-divisions, or of anything having its mark (such as A*b*C*d*=0), is called a simple statement. A denial of two or more cross-divisions is called a compound statement, and moreover two-fold, three-fold, etc., according to the number denied.

When two compound statements can be converted into one another by interchange of the classes A,B,C,D with each other or with their complementary classes *a*,*b*,*c*,*d*, they are called *similar*; and all similar statements are said to belong to the same *type*. The problem before us is to enumerate all the types of compound statement that can be made with four terms.

2. Two statements are called *complementary* when they deny between them all the sixteen marks without both denying any mark; or, which is the same thing, when each denies just those marks which the other permits to exist. It is obvious that when two statements are similar, the complementary statements will also be similar; and, consequently, for every type of n-fold statement there is a *complementary type* of $\overline{16-n}$-fold statement. It follows that we need only enumerate the types as far as the eighth order; for the types of more-than-eight-fold statement will already have been given as complementary to types of lower orders. Every eight-fold statement is complementary to an eight-fold statement; but these are not necessarily of the same type.

3. One mark ABCD may be converted into another A*b*C*d* by interchanging one or more of the classes A,B,C,D with its

complementary class. The number of such changes is called the *distance* of the two marks. Thus in the example given the distance is 2. In two similar compound statements the distances of the marks denied must be the same; but it does not follow that when all the distances are the same, the two statements are similar. There is, however, as we shall see, only one example of two dissimilar statements having the same distances. When the distance is 4, the two marks are said to be *obverse* to one another, and the statements denying them are called *obverse* statements; as ABCD, *abcd;* or, again, A*b*C*d*, *a*B*c*D. When any one mark is given (called the *origin*), all the others may be grouped in respect of their relations to it as follows. Four are at distance *one* from it, and may be called *proximates;* six are at distance *two*, and may be called *mediates;* four are at distance *three*, and may be called *ultimates.* Finally, the obverse is at distance *four.*



origin and 4 proximates. 6 mediates. obverse and 4 ultimates.

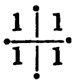
It will be seen from the above table that the four proximates are respectively obverse to the four ultimates, and that the mediates form three pairs of obverses. Every proximate or ultimate is distant 1 and 3 respectively from such a pair of mediates. Thus each proximate or ultimate divides the mediates into two classes; three of them are at distance 1 from it, and three at distance 3. Two mediates which are not obverse are at distance 2. Two proximates, or two ultimates, or an ultimate and a proximate which are not obverse, are also at distance 2.

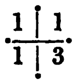
This view of the mutual relations of the marks is the basis of the following enumeration of types.

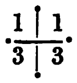
4. There is clearly only one type of simple statement. But of two-fold statements there are four types; viz. the distance may be 1, 2, 3, or 4, and so in general with n classes there are n types of two-fold statement.

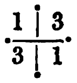
5. A compound statement containing no pair of obverses is called *pure*. In a three-fold statement there are three distances; one of these must be not less than either of the others. If this be 2, the remaining mark must be at odd distance from both of these or at even distance from both; thus we get the types 1, 1, 2, and 2, 2, 2. If the not-less distance be 3, the remaining distances must be one even and the other odd; the even distance must be 2, the odd one either 1 or 3, and the types are 1, 2, 3; 2, 3, 3. Thus there are 4 pure three-fold types; with a pair of obverses, the remaining mark must be at odd or even distance from them; 1, 3, 4; 2, 2, 4. In all six three-fold types; observe that there is necessarily one even distance.

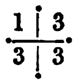
6. *A fortiori*, in a four-fold statement there must be one even distance. In a pure four-fold statement this distance is 2. From this pair of marks let both the others be oddly distant; then they must be evenly distant from one another, *i.e.* at distance 2, obverses being excluded. The odd distances are 1 or 3, and it will be easily seen that the following are all the possible cases:

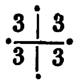












In these figures the dots indicate the four marks, the cross lines indicate distance 2, and the other figures the distances between the marks on either side of them. Next, from the pair of marks at distance 2 let one of the others at least be evenly distant, *i.e.* at distance 2. Then we have three marks which are all at distance 2 from one another, and it is easy to show that they are all proximates of a certain other mark. For select one of them as origin; then the other two are mediates which are not obverse, and which consequently are at distance 1 from some one proximate. With this proximate as origin, therefore, all three are proximates. We have therefore only to enquire what different relations the fourth mark can bear to these three. It may be the origin, its obverse, the remaining proximate, its obverse, or one of two kinds of mediates, viz. at distance 1 or 3 from the remaining proximate. Thus we have 6 types, in which the distances of the fourth mark from the triad are respectively 111, 333, 222, 222, 133, 113. The third and fourth of these are especially interesting, as being distinct types with the same set of distances; I call them *proper* and *improper groups* respectively, viz. a proper group is the four proximates of any origin, an improper group is three proximates with the obverse of the fourth. On the whole we get 12 types of pure four-fold statement.

7. In a four-fold statement with *one* pair of obverses, take one of them for origin; the remaining two marks must then be either a pair of proximates or ultimates, a proximate and an ultimate, a pair of mediates, or a proximate or ultimate with one of two kinds of mediate; in all 5 types, with the distances $13^2,13$; $13^2,31$; $22^2,22$ $13^1,22$; $13^3,22$. With *two* pair of obverses, they must be either at odd or even distances from one another; two types. Altogether $12+5+2=19$ four-fold types.

8. In a *pure* five-fold statement there is always a triad of marks at distance 2 from one another. For there is a pair evenly distant; if there is not another mark evenly distant from these, the remaining three are all oddly distant, and therefore evenly distant from one another. First then let the remaining two marks be both oddly distant from the triad. In regard to the origin of which these are proximates, the two to be added must be either two mediates, like (of two kinds) or unlike, or a mediate of either kind with the origin or the obverse; 7 types. Next,

if one of the two marks be evenly distant from the triad, it must form with the triad either a proper or an improper group of four. To a proper group we may add the origin, the obverse, or a mediate; to an improper group, the origin or the obverse (the mediates give no new type), 5 types, or in all 12 pure five-fold types.

9. In a five-fold statement with one pair of obverses, there must be another pair of marks at distance 2. We have, therefore, to add one mark to each of the following three types of fourfold statement; a pair of obverses together with (1) two proximates, (2) a proximate and an ultimate, (3) two mediates. To the first we may add another proximate, an ultimate, or a mediate of 3 kinds, viz., at distances 11, 13, 33 from the two proximates; 5 types. To the second, if we add a proximate or an ultimate, we fall back on one of the previous cases; but there are again three kinds of mediates, at distances 11, 33, 13 from the proximate and ultimate; 3 types. To the third we may add another mediate, whereby the type becomes a proper group together with the obverse of one of its marks, which is the same thing as an improper group together with the obverse of one of its marks; or a proximate or ultimate, which are of 3 kinds, at distances 11, 13, 33 from the two mediates; 4 types. Thus there are 12 five-fold types with one pair of obverses. With two pairs of obverses at odd distances there is only one type, all the remaining marks being similarly related to them; at even distance, the remaining mark may be evenly or oddly distant from them; 2 types. On the whole we have 12+12+3=27 types of five-fold statement.

It is to be remarked that there is no pure five-fold statement in which all the distances are even; and that if there is only one pair of obverses with all the distances even, the type is a proper group together with the obverse of one of its marks.

10. We may now prove as a consequence of the last remark that a pure sixfold statement either contains a group of four with a pair oddly distant from it or consists of two triads oddly distant from one another. For there must be a pair at distance 2; if the other four are all oddly distant from these, they form a group; if one is evenly distant and three oddly distant, we have the case of the two triads; if two are evenly distant, we again have a group. We must add then first to a proper group and then to an improper group a pair oddly distant from it. To a proper group, consisting of the proximates to a certain origin, we may add the origin or its obverse with a mediate, or two mediates; 3 types. An improper group is symmetrical; that is to say, if we substitute for any one of its marks the obverse of that mark, we shall obtain a proper group. In this way we shall get four origins, distant 1113 from the group, and four obverses distant 1333; if we add to these the obverses of the marks in the group itself, we have described the relation of the twelve remaining marks to the group. To form therefore a pure six-fold statement we may add either two origins or two obverses or an origin and an obverse; 3 types.

In the case of the two triads, since they are oddly distant from one another their origins must be oddly distant, that is, they must be distant either 1 or 3. If they are distant 1, neither, both, or one of the origins may appear in the statement; if they are distant 3, neither, both, or one of the obverses; 6 types. Thus we obtain 12 types of purely six-fold statement.

11. If a six-fold statement contains one pair of obverses, the remaining four marks cannot all be evenly distant from this pair. For in that case they would constitute a group; and it is easy to see that the marks evenly distant from a group, whether proper or improper, do not contain a pair of obverses. We have therefore only these four cases to consider:

(1) the four marks are all oddly distant from the obverses;
(2) one is evenly distant and three oddly distant;
(3) two are evenly distant and two oddly;
(4) three are evenly distant and one oddly.

In the first case the four marks form a group. If this is a proper group, the pair of obverses must be either the origin and obverse of the group, or a pair of mediates; 2 types. If the group is improper, the pair must be an origin and an obverse; 1 type. In the second case, we have an origin, an obverse, and a mediate, to which we must add 3 marks taken out of the proximates and ultimates. We may add 3 proximates distant respectively 113 or 133 from the mediate; 2 types, or 2 proximates distant respectively 11, 13, 33 from the mediate and with each of these combinations an ultimate distant either 1 or 3; 6 types. To interchange proximates with ultimates clearly makes no difference; so that in reckoning the cases of 1 proximate and 2 ultimates or 3 ultimates, we should find no new types. In the third case we have an origin an obverse and two mediates, distant 2 from each other, and to these we have to add either two proximates or a proximate and an ultimate. The two proximates may be distant from the two mediates 11, 13, or 11, 33, or 13, 13, or 13, 33; 4 types. The proximate and ultimate must not be respectively distant 11, 33, or 33, 11, for then they would form a pair of obverses; there remain the cases 11 with 11 or 13, 13 with 13, and 33 with 13 or 33; 5 types. In the fourth case we have an origin obverse and 3 mediates distant 2 from one another; the remaining mark must be distant either one or three from these mediates; 2 types. This makes 22 types of six-fold statement with 1 pair of obverses.

12. If a six-fold statement contain two pairs of obverses these must be either evenly or oddly distant. If they are evenly distant, we have an origin obverse and two obverse mediates, to which two other marks are to be added. These

may be both evenly distant; taking one of them as origin, it is associated with 5 mediates, so that there is one type only. Or both oddly distant; here there are two cases, according as the distances are 11, 33 or 13, 13. Or one oddly and one evenly distant; the latter is any one of the four remaining mediates, and then the former is distant 1 or 3 from it; 2 types. If the two pairs of obverses be oddly distant, they form an aggregate which is related in the same way to all the remaining 12 marks; viz. any one of these being taken as origin, we have a pair of mediates and a proximate with its obverse ultimate. The thing to be considered, therefore, is the distance between the two marks to be added, which may be 1, 2 or 3, and each in two ways; 6 types.

A six-fold statement with 3 pairs of obverses is one of two types only; viz. these are all evenly distant, when they are the mediates to one origin, or two evenly distant and one oddly distant from both of them.

13. A pure seven-fold statement must consist of a group and a triad; for it must contain a triad, by the same reasoning by which this was proved for a five-fold statement, and then either all the other four marks are oddly distant from this, and so form a group by themselves; or else one of them is evenly distant from the triad and so forms a group with it. If the group is proper, being the proximates to a certain origin, the triad must consist of two mediates and either the origin, the obverse or another mediate, and in the latter case the 3 mediates are distant 111 or 333 from some proximate; 4 types. If the group is improper, the triad is either all origins or all obverses or two origins and an obverse or an origin and two obverses; 4 types. In all 8 types of pure seven-fold statement.

14. A seven-fold statement with one pair of obverses must consist either of four marks evenly distant from one another and three oddly distant from them; or of five marks evenly

distant from one another and two oddly distant from them. In the former case the pair of obverses may be in the four or in the three. If they are in the four, the three form a triad which are proximates to one origin, and then the pair may be the origin and obverse or a pair of mediates. If the pair are origin and obverse, the other two (at distance 2) are mediates, distance 11, 13 or 33 from the proximate which is not in the triad; if the pair are mediates, the two may be the origin or obverse with a mediate distance 1 or 3 from that proximate (4 types) or two mediates distant 11, 13, 33 from it (3 types). If the pair of obverses are in the set of three marks the four form a group, which may be proper or improper. If proper the three may be origin and obverse with a mediate, or a pair of mediates with origin, obverse, or another mediate; 4 types. If improper, the three must be two origins and an obverse or an origin and two obverses; 3 types.

Five marks evenly distant containing only one pair of obverses, must be a proper group with the obverse of one of its marks; see end of art. 9. To these we may add the origin or obverse of the proper group with a mediate distant 1 or 3 from the extra mark, or else two mediates distant 11, 13 or 33 from that mark; 7 types.

15. A seven-fold statement with two pairs of obverses may have six marks evenly distant from one another and one oddly distant from them; in this case the six are an origin and five mediates in two different ways, or say two pairs and a two; the remaining mark may be distant 11, 13 or 33 from the two, which gives 3 types.

Otherwise the seven-fold statement must subdivide (as in the last case) into five and two or into four and three. If it subdivide into five and two, the two may be a pair or not. In the first case we have a proper group and the obverse of one of its marks, together with the origin and obverse of the group or a pair of mediates; two types. In the second case we have five mediates of an origin or its obverse, to which we may add 2 proximates distant 11, 13 or 33 from the odd mediate, or a proximate and an ultimate distant 11, 13 or 33 respectively from the odd mediate; 6 types.

If the seven-fold statement subdivide into four and three, the two pairs may be both in the four, or one in the four and one in the three. In the former case we have a triad, to which may be added the origin and obverse and a pair of mediates, or two pairs of mediates; two types. In the latter case the four consist of an origin and obverse and two mediates; we must add a pair consisting of a proximate and an ultimate, which may be distant 11, 33 or 13, 13 from the two mediates, and then another proximate or ultimate which may be distant 11, 13, or 33 from the two mediates; 6 types.

16. Three pairs of obverses in a seven-fold statement may be all evenly distant, or two evenly and the other pair oddly distant from each. If they are all evenly distant they are the mediates to a certain origin or its obverse, and the seventh mark may be the origin or a proximate, 2 types. In the other case we have an origin obverse and pair of mediates together with a proximate and its obverse ultimate; we may add a proximate or a mediate, 2 types.

17. A pure eight-fold statement must consist of two groups, either both proper or both improper, or one of each. Two proper groups may have their origins distant 1 or 3; two types. To an improper group we may add a proper group made of one origin and three obverses, or of three origins and one obverse; or an improper group made of four origins or four obverses, or two origins and two obverses; five types; altogether there are seven types of pure eight-fold statement.

18. An eight-fold statement with one pair of obverses must subdivide into four and four, or into five and three.

In the former case we have a pair of obverses, viz. an origin and its obverse, and two mediates; to which we must add a group formed out of the proximates and ultimates. This group may be proper, one type, or improper, the mediates being in regard to it two origins, two obverses, or an origin and an obverse; three types. In the latter case the five marks must be a proper group with the obverse of one mark, to which we must add a triad made out of the origin, obverse and mediates of the group. This triad may be the origin or obverse together with two mediates distant 11, 13, 33 from the ultimate, six types; or else it may be three mediates distant 111, 113, 133, 333 from the ultimate; four types.

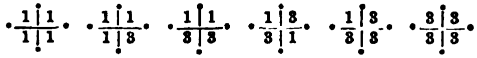
19. An eight-fold statement with two pairs of obverses must subdivide into four and four, or into five and three, or into six and two. In the first case the two pairs of obverses may be evenly distant, when the remaining marks form a group either proper, with its origin, obverse and pair of mediates, or two pair of mediates, or else improper, 3 types; or oddly distant, when the remainder form one of the 6 pure four-fold statements enumerated art. 6. Two marks distant 2 from each other may be distant 11, 33 or 13, 13 from the pair of obverses which is oddly distant from them; thus each of the 6 four-fold statements gives 3 types of eight-fold statement, except the third, which gives 4; in all 19. In the second case the three may be a triad, or may contain a pair of obverses. If it is a triad, the five are mediates to one origin and its obverse, and we may add three proximates distant 113 or 133 or two proximates distant 11, 13 or 33 with an ultimate distant respectively 1, 1 or 3, 3, from the odd mediate, 6 types. If the three contain a pair of obverses, the five make a proper group with obverse of one mark; to this we may add the origin and obverse of the group with mediate distant 1 or 3 from the ultimate, or a pair of obverse mediates with a mediate distant 1 or 3 as before, 4 types. In the third case the six must be an origin and five mediates, and

we may add two proximates distant 11, 13, 33 from the odd mediate, or a proximate and an ultimate, or two ultimates, distant as before, 9 types.

20. In an eight-fold statement with three pairs of obverses these may be either all evenly distant, or two of them evenly distant and the other oddly distant from both. In the first case they are mediates to a certain origin and its obverse, and we may add the origin with a proximate or ultimate, two proximates, or a proximate and ultimate, 4 types. In the second case take the oddly distant pair for origin and obverse, then these are associated with two proximates and their obverse ultimates, and we may add the two other proximates, a proximate and an ultimate, a proximate and a mediate (distant 11, 13, 31, 33 from this proximate and the remaining one) or two mediates distant 11, 33 or 13, 13 from the two proximates, 8 types.

Lastly, in an eight-fold statement with four pair of obverses they may be all evenly distant, or the statement may subdivide into six and two, or into four and four; in the latter case there are two types.

21. To obtain the whole number of types, we observe that for every less-than-eight-fold type there is a complementary more-than-eight-fold type (art. 2); so that we must add the number of eight-fold types, 78, to twice the number of less-than-eight-fold types. 159; the result is 396.

Art.	TABLE.			
4	1-fold			1
	2-fold, distance 1, 2, 3, 4			4
5	3-fold, pure, distance 112, 222, 123, 233		4	
	1 pair obv., dist. 134, 224		2	
			6	6
6	4-fold, pure, two and two—			
		6		
	three and one	4		
	group, proper or improper	2		
		12	12	
7	1 pair obv		5	
	2 pair obv., dist. odd or even		2	
			19	19

Art.	TABLE.			
8	5-fold, pure, three and two	7		
	four and one	5		
			12	
9	1 pair obv. + two prox. +	5		
	+ prox. + ult. +	3		
	+ two med. +	4		
		12	12	
	2 pair obv., odd dist., 1; even, 2		3	
			27	27
10	6-fold, pure, three and three	6		
	four and two	6		
		12	12	
11	1 pair obv., two and four	3		
	three and three	8		
	four and two	9		
	five and one	2		
		22	22	
12	2 pair obv., odd dist., 6; even, 5		11	
	3 pair obv.		2	
			47	47
13	7-fold, pure; proper group, 4; improper, 4		8	
14	1 pair obv., four and three	10		
	three and four	7		
	five and two	7		
		24	24	
15	2 pair obv., six and one	3		
	five and two	8		
	four and three	8		
		19	19	
16	3 pair obv.		4	
			55	55
	Total of less-than-eight-fold statements			159
	Complementary more-than-eight-fold statements			159
17	8-fold, pure		7	
18	1 pair obv., four and four	4		
	five and three	10		
		14	14	
19	2 pair obv., four and four	22		
	five and three	10		
	six and two	9		
		41	41	
20	3 pair obv., all evenly dist.	4		
	two evenly ,,	8		
		12	12	
	4 pair obv.		4	
			78	78
	Grand Total			396

The following paper was read at the Meeting of the Society held on the 26th December, 1876.

"A Notice of some Organic Remains from the Schists of the Isle of Man," by E. W. BINNEY, President, F.R.S., &c.

Many years since Dr. Berger, Mr. Wood, Professor Henslow, and Dr. Macculloch, noticed these deposits; but it is chiefly owing to the researches of the Rev. J. C. Cumming that we are indebted for a more detailed account of them. In his Guide to the Isle of Man, published in 1861, he states that "the greater portion of the island, probably three-fourths, consists of rocks which it may be convenient at present to include under the general term of *Cambro-Silurian*," intending thereby all the rocks under the Upper Silurian of Sir R. J. Murchison.

"This statement must be taken cautiously, as resting partly on negative evidence, in the absence of determinate fossils, and partly on direct lithological considerations. No fossils, with the exception of a few undetermined fucoids or corallines, have, as yet, been discovered in these rocks, but by diligent search they may hereafter be obtained; yet they can be very well compared in lithological character with that series of rocks on the western borders of Shropshire and eastern borders of Montgomeryshire, to which the name of 'Lower Silurian' was long since given by Sir R. Murchison, together with, perhaps, the true Cambrians of the Longmynds. They consist of soft clay schists, often highly ferruginous, and sometimes mottled, with intercalated bands of harder and siliceous rocks, and, which is most interesting, in comparing them with the Corndon series, with bands of felspathic greenstone, the outpouring of which and formation of beds must have been contemporaneous with the deposit of the mud of which the clay schists are made up. These phenomena are nowhere better developed than on the peninsula of Langness and all along the eastern coast from Cass-na-awin (at the mouth of the Santon burn) to Maughold Head.

"These schists have been highly contorted by the subsequent intrusion of granites and porphyries, but their general dip is to the N.W. and S.E. of the range of mountains running from the Calf of Man to North Barrule, which has a general direction from S.W. to N.E. corresponding with the strike of the schists.

"The granite has protruded at Foxdale, on the eastern side of South Barrule, and at the Dhoon, north of Laxey, greatly altering the schists where in contact with them. The age of these granite bosses is uncertain.

"In the neighbourhood of the granites the most productive mineral veins have been discovered. Large masses of white quartz lie along the strike of the schists, and a vein of it cutting through the eastern side of the granite of Foxdale is at least 30 feet thick. The thickness of these old schists is enormous, and must be calculated at many thousands of feet, making every allowance for the existence of faults, for we may walk across the upturned edges, and trace them layer imposed upon layer through great distances. We have one small portion of them in the form of a blue fibrous and elastic flagstone at Spanish Head and the Chasms lying almost horizontal, and having a vertical thickness of more than 300 feet.

"There are no distinct evidences of true slaty cleavage in these schists; the partings in the so-called slates of South Barrule being in the direction of the bedding. An *imperfect* slaty cleavage appears in some instances to have originated joints by which the rock is broken up into rhomboidal forms."

In the *Geologist* for May, 1862, Mr. Thomas Grindley describes these deposits as Lower Cambrian, and states that few organic remains had been met with in them, except some imperfectly preserved fucoids and corals, and he estimates the thickness of the strata from 7 to 10,000 feet.

In vol. iv., p. 74, of the Transactions of the Manchester Geological Society, Mr. John Taylor, F.G.S., describes the Manx schists as of Cambrian age, and states that in the quarries below the Castle Mona Hotel, Douglas, he had discovered both fucoids and the tracks or castings of worms.

Professors Harkness and Nicholson, in a paper printed in the *Quarterly Journal*, vol. xxii., p. 488, come to the conclusion that the Manx schists are of Silurian age, and consist of green slates and porphyries, and Skiddaw slates, and in confirmation of their opinion as to the latter deposits, state that they had found, on the north of Douglas Bay, specimens of *Palæochorda major.*

Mr. John Horne, F.G.S., of the Geological Survey of Scotland, in a memoir printed in vol. ii., part iii. of the Transactions of the Geological Society of Edinburgh, in writing on the Silurian rocks of the island, states "that nearly four-fifths of the island are made up of rocks belonging to this formation. A chain of high ground extends from Ramsay to the Calf of Man. This chain, running from N.E. to S.W., forms the backbone of the island, and is approximately the strike of the Silurian rocks. These rocks, so far as observed by me, consist of indurated grey slaty shales and flags. In many places they show imperfect cleavage. In Glen Moar, and on Slieu Dhoo, in the Sulby Glen and its tributaries, and on the top of Snaefell, the general dip varies from 20° to 40° N. of W. Again, in the south of the island, in Silver Burn, at the cliffs near Port St. Mary, and on the eastern side of Langness the general dip is from 30° to 40° E. of S. at a high angle. From these dips it is evident that a well-marked anticline exists in these rocks. Several years ago Professors Harkness and Nicholson pointed out that this axial line runs from Port Erin to Port Moar, near Maughold Head. The grey slaty shales and flags which occur on both sides of the great anticline were considered by them to be the equivalents of the Skiddaw slates of Cumberland. They based this conclusion partly on the lithological resemblance, partly on the fact that the beds in the two localities are in the same line of strike, and also on the discovery of *Palæochorda major*, a fossil very abundant in the Skiddaw slates of Cumberland.

"At Douglas Head, and northward as far as Clay Head, they found certain green slates and porphyries which they considered to be on the same horizon as the green slates and ash beds of the Lake country.

"If this correlation be correct, as all the available evidence seems to prove, then it follows that the grey slaty shales and flags of the Isle of Man are of Lower Llandeilo age. They are inferior in position to the Silurian rocks of the south of Scotland. Lithologically they are totally different from any part of that series. The green slates and porphyries are in all probability of Lower Caradoc age. In this locality there seems to be no break between the two series, for they rest conformably on the grey slaty shales and flags."

So far as I have observed, these schists consist chiefly of argillaceous and arenaceous shales, generally of a brown but sometimes of a bluish and occasionally of a greenish colour. Beds of ash are met with in them, but they afford very few calcareous bands. For the most part they appear to have been elevated to the high angle at which they are now found by the raising up of the central chain of hills running from the Calf to North Barrule. Where they are found lying level they present a different appearance from that shown when they are found lying nearly vertical or contorted. In the former case they are comparatively soft and plastic, but in the latter the argillaceous beds are often hardened into fibrous slates, whilst the arenaceous beds have been converted into an impure quartzite. This appearance is well shown in the quarries near the gasworks at the south side of Douglas Harbour, and is probably due to the great heat which would necessarily be developed by the moving and bending of the beds after they had been consolidated, without resorting to any other cause. In the neighbourhood of the granites of Foxdale and Dhoon, however, the adjoining schists are converted into gneissic and micaceous rocks, and exhibit all the appearances of metamorphic action, and probably these granites, especially the latter, may also be proved on more careful examination to be due to that cause.

No measurements have been made of the thickness of the schists, nor have any marked characters been observed so as to divide them with certainty, with the exception of a bed of dark blue slates which I have traced running from Oakhill, in Braddan, past Port Soderic Railway Station to Mary Veg, in Santon. Above these strata lie the schists of Walbury, Douglas Head, and Banke's Howe, of great thickness, and certainly a much greater thickness lies below, between them and the granite at Foxdale. It is very desirable to obtain a base line, especially if it is marked by organic remains, to assist in measuring and classifying the beds.

All the authors above quoted notice the absence of organic remains with the exception of undetermined fucoids and the *Palæochorda major* observed by Professors Harkness and Nicholson. For myself, after some years' search, I have not been able even to find the above-named specimens in such a state of preservation as to be certain of their nature, without it be a fucoid in my possession found by Mr. Grindley in the drift near Laxey. The first place where I thought that I had obtained traces of organic remains was in the grey flaggy slates on the shore near Derby Castle, on the south side of Douglas Bay. They consisted of circular and oval shaped holes on the surface of the rock, and reminded me of *Arenicolites*, but as they occurred only a little above and a little below high water mark, and I could not find them down to low water mark, as they ought to have been if they had been made by the sea on soft places in the rock, they probably are due to the action of boring molluscs, or some other cause, and have not been made by ancient annelides. But certainly *Scolites* similar in character although much larger in size occur in these last-named beds, greatly resembling those mentioned by the late Mr. Salter as occurring in the hard sandstone at the base of the Lower Llandeilo shales, near Tremadoc, and described by him in the Memoirs of the Geological Survey, vol. iii. p. 292, pl. x. fig. 27.

Great difficulties no doubt occur in distinguishing the burrows and castings of marine worms from the tracks and trails of bivalve and univalve shells or of a lgæ, andin the present case, in determining the fossils, I take them to be *Nemerites* and *Nereites* from their, in my opinion, resembling those genera more than any other with which I am acquainted. The absolute identification of my specimens with those given of Sir R. Murchison's being by no means clearly made out.

In making the railway from Douglas to Castletown a bed of laminated slates, of a dark blue colour, dipping at a high angle to S.E., was cut through at Oakhill, in Braddan. It was several hundred feet in thickness, and occurred between two deposits of grey shales. In this dark blue bed were found the burrows of large and small worms. Amongst the former I found the specimen plate i., fig. 1, of a loop shape, 12 inches long, and ⅛th-inch in diameter. It occurred on the fragment of stone as drawn, so I could not tell whether or not it had ever been connected with other loops. In comparing it with the *Nemerites Olivantii* of Murchison it bears considerable resemblance to that fossil, but as it is evidently only a portion of a specimen it is difficult to identify thoroughly, so it is proposed to call it *Nemerites Monensis*. Besides this specimen there are some oval shaped bodies about an inch long and half an inch broad, with a thin tail-like appendage, one and a half inch distant from each other. Plate i., fig. 2 gives a representation of one of them. Similar forms from the schists of Laxey and Dalby have been supposed by Messrs. Grindley and Taylor to be casts of the footprints of animals, but to me they appear more like the casts and trails of a bivalve shell in soft mud. In size and shape they resemble the *Linguella Davisii* of Mc.Coy.

In an old quarry below Port Soderic Railway Station the blue slates are again met with, dipping to the S.E., and the tracks and burrows of annelides similar to those found at Oakhill occur.

Further to the south of the last-named locality, in a new slate quarry on the cliff above the sea at Mary Veg, in Santon, the blue slates are now being wrought. They dip to the S.E., at an angle of 60°. In them I found the meandering worm-like body represented in plate ii., fig. 1, both in mould and cast, the latter being on the left-hand side of the upper portion of the specimen. In the former (the mould) it is of an S shape at the lower part of the stone, where it appears to consist of about one hundred segments, with traces of feet and cirri not well shown, and terminating in what appears to be like an oval-shaped head. In the form of its curve and size it agrees pretty well with *Nereites Cambrensis,* but in the shape of the feet it somewhat resembles *Nereites Sedgwickii* of Murchison, so being different from both of these species it is proposed to call it *Nerites Monensis.*

The two small specimens described in plate ii., fig. 2, are of irregular oval shape, about three-quarters of an inch long and half an inch broad. They occur in considerable numbers in some of the beds, but to what they belonged it is difficult to say, without they are the open valves of a *Linguella* or some other bivalve shell.

The occurrence of *Nemerites* and *Nereites,* or bodies resembling them, in the blue slates, similar to those found in the Lower Llandeilo beds at Llampeter, appears to show that these Manx schists are of Lower Llandeilo age as suggested by Mr. Horne.

The blue slates in which the specimens of *Nemerites* and *Nereites* are found, and the beds near Derby Castle, containing *Scolites,* from their organic remains, may therefore be taken as the Manx representatives of the Lower Llandeilo beds; but what the several thousand feet of strata lying above them at Walbury, Douglas Head, and Banke's Howe are, we have at present no evidence from fossils to enable us to decide with certainty. From Mary Veg, and the other localities where the blue slates containing *Nereites* and *Nemerites* are found to the mica schists and gneissic rocks, near Foxdale, is fully four miles on the rise and dip, and the underlying strata, as far as exposed, incline to the S.E. at a high angle; so, after allowing for faults, probably some thousand feet of strata lie under the Lower Llandeilo beds. Whether these are all Lower Silurian, or some of them Cambrian, remains yet to be proved; but it is apparently by no means improbable that Mr. Cumming, and others, may not have been mistaken in supposing some of the lower beds to be of Cambrian age.

I am indebted to Mr. H. H. Bailey, F.G.S., Acting Palæontologist to the Irish Geological Survey, for his kindness in drawing the specimens.

EXPLANATION OF PLATES.

Plate I.

Fig. 1.—Specimen of *Nemerites? Monensis*, from Oakhill, in Braddan; natural size.

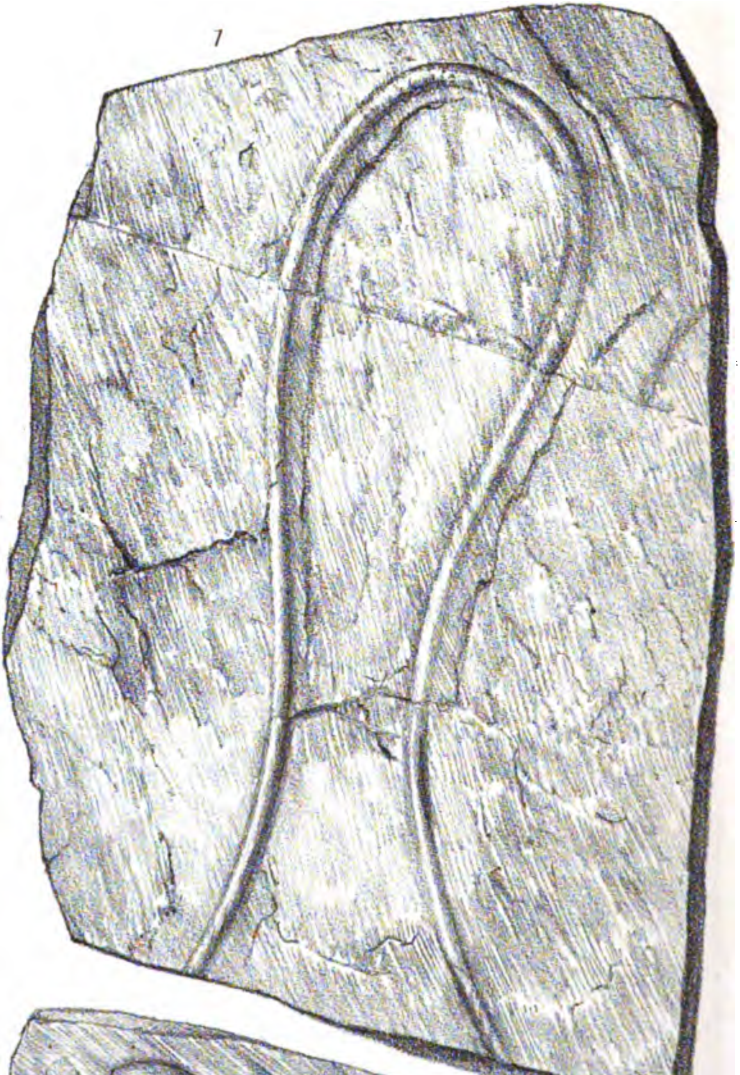
Fig. 2.—*Linguella Davisii?* (Mc.Coy), from the same locality; natural size.

Plate II.

Fig. 1.—Specimen of *Nereites? Monensis*, from the slate quarry of Mary Veg, in Santon; natural size.

Fig. 2.—Oval valves of a bivalve shell? from the same locality; natural size.

1



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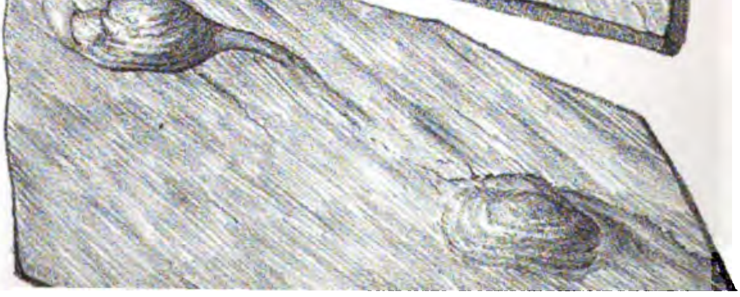

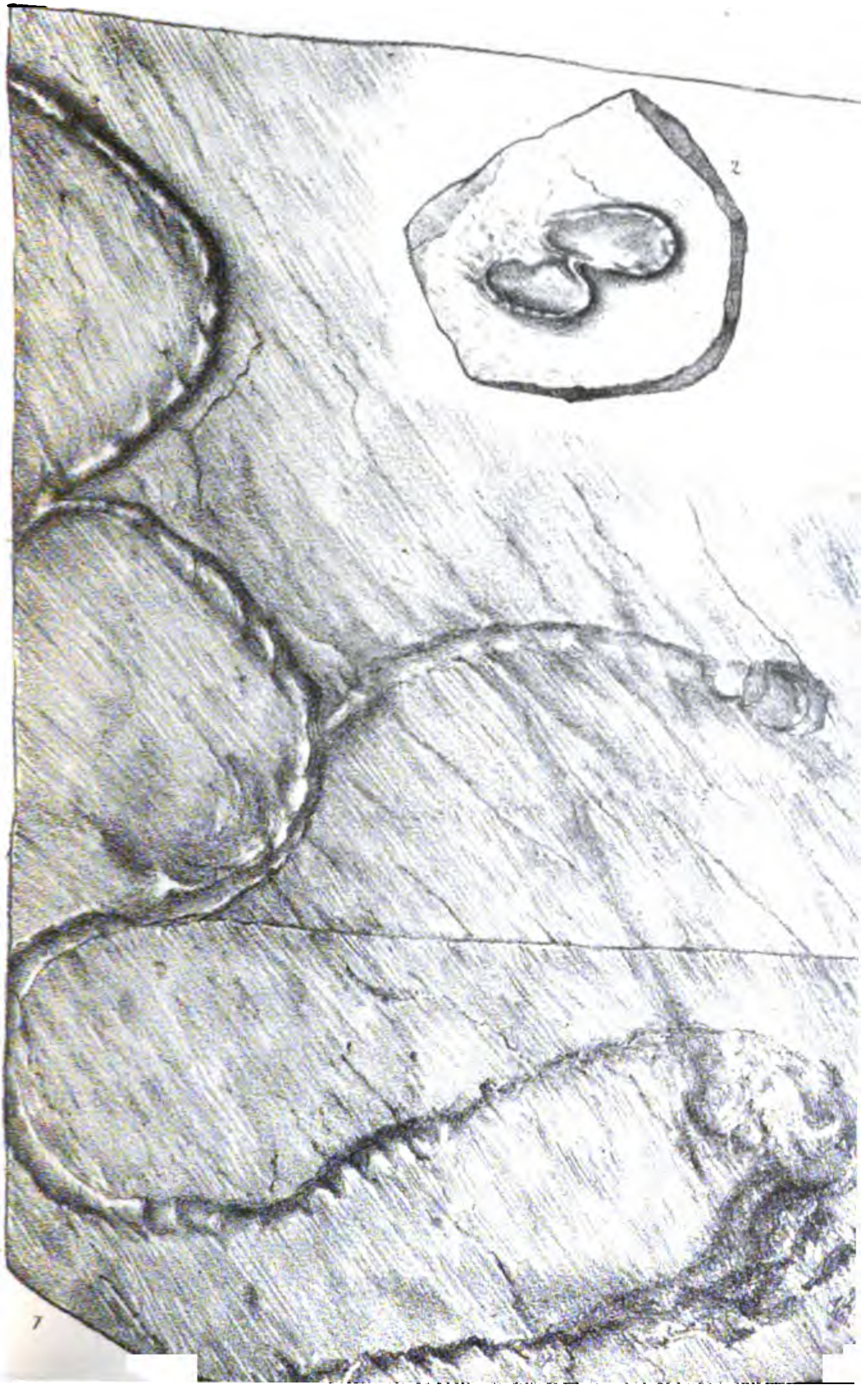
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General Meeting, February 6th, 1876.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

Mr. John Henry Poynting, B.A., B.Sc., Demonstrator in the Physical Laboratory, Owens College, and Mr. William Garnett, of Quernmore Park, Lancaster, were elected ordinary members of the Society.

Ordinary Meeting, February 6th, 1877.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

"Notice of the Junior Literary and Philosophical Society of Manchester, 1806-1807," by William E. A. Axon, M.R.S.L.

Mr. Axon presented to the Library of the Society a small manuscript volume formerly belonging to Alderman John Shuttleworth. It was the minute-book of a Junior Literary and Philosophical Society formed April 29th, 1806, the original members being Messrs. Peter Clare, Kinder Wood, Godfrey Fox, E. R. Kent, —— Hall, Frith Jordan and and Goodlad, subsequent members being Messrs. Thomas Hanson, Thomas Philips, John Oliver, John Shuttleworth, Edwin Watt, John Hodson, John Lowe, John Stevenson. The meetings were held fortnightly in the chapel in Cross-street, the members being required to give a lecture or paper in rotation, any member coming after seven to be fined 6d., if absent entirely to pay 2s. 6d. The society was to meet for general literary purposes, those branches of the art of Medicine not coming under the heads of anatomy, physiology, and chemistry, were excluded. This restriction was afterwards abolished.

At the second meeting something unpleasant must have happened, which resulted in the resignation of three of the founders. The society appears to have been largely medical in its composition, and the first paper was an anatomical demonstration of the brain by Mr. Kinder Wood. The fines for papers not produced are numerous. At the eleventh assembly it was resolved that in consequence of there being only two members present the meeting be adjourned. At the fifteenth meeting it was agreed that every member when he addressed the meeting "shall deliver his sentiments standing." The author of a paper was however allowed in this respect to please himself. On November 13, "the meeting was called earlier than usual in consequence of Mr. Dalton's lectures happening on the Wednesday evening to accommodate those members who attend them." On Jan. 14, 1807, it was resolved unanimously that a special meeting be convened at the House of Recovery, "to take into consideration the irregularity of the attendance of some of the members." Considering the "parlous state" of the small association, there is an evident appropriateness in the locality selected. At this special meeting it was decided to send a letter to Mr. Moorhouse, "expressing in strong terms the dissatisfaction the society cannot fail from experiencing from the want of attendance he has uniformly shown." Perhaps the absentee got wind of this, for he sent in his resignation before the letter was sent; but the Secretary was desired to call upon him for the fines he had incurred. Mr. HALL read a paper on Taste, which was discussed. Whether anything disagreeable was said is not recorded, but two meetings subsequently there is a record of his resignation being brought before the society, "but in consequence of his letter reflecting very unjustly upon the members of this society it was resolved it be returned to him and if a satisfactory explanation be not given, then his name be erased from the list with disgrace." This severe course was acted upon in relation to another member, but whether any evil results accrued does not appear. The last entry in the volume is that of the 43rd general meeting, 23rd October, 1807, when Mr. Peter Clare was re-elected President, Mr. Godfrey Fox, Vice-President, and Mr. John Hodson, Secretary. The papers brought before the meetings included communications on "History," "Human Passions," "Galvanism," "General Laws of Matter," "Inhuman Treatment from Man to Animals," "Temperance," "Mechanics," "Comparison between Ancient and Modern History," "Present State of Morals," "Electricity," "Eloquence." This last was the essay of Mr. John Shuttleworth, and it is grievous to find that only one member and two visitors attended. It was read however at the succeeding meeting to an audience of six. Whether there are any more records of this society in existence is not known. Probably the society died without a pang, and hence the minute-book would naturally remain in the hands of its former Secretary.

"On Compound Combinations," by Prof. CAYLEY, F.R.S., &c.

Prof. Clifford's paper, "On the Types of Compound Statement involving Four Classes," relates mathematically to a question of compound combinations; and it is worth while to consider its connection with another question of compound combinations, the application of which is a very different one.

Starting with four symbols, A,B,C,D, we have 16 combinations of the *five types* 1,A,AB,ABC,ABCD ($1+4+6+4+1=16$ as before); but in Prof. Clifford's question 1 means A′B′C′D′, A means AB′C′D′, &c., viz., each of the symbols means an aggregate of four assertions; and the 16 symbols are thus *all of the same type*. Considering them in this point of view, the question is as to the number of types of the binary, ternary, &c., combinations of the 16 combinations; for, according as these are combined

No. of types is $=\frac{1,2,3,\ 4,\ 5,\ 6,\ 7,\ 8,\ 9,\ 10,11,12,13,14,15}{1,4,6,19,27,47,55,78,55,47,27,19,6,\ 4,\ 1}$ together

In the first mentioned point of view the like question arises, in regard to the sets belonging to the 5 different types separately or in combination with each other; for instance, taking only the 6 symbols of the type AB, these may be taken 1,2,3,4, or 5 together, and we have in these cases respectively

$$\text{No. of types}=\frac{1,2,3,4,5}{1,2,2,2,1}$$

as is very easily verified; but if the number of letters A,B... be greater, say this is=8, or, instead of letters, writing the numbers 1,2,3,4,5,6,7,8, then the question is that of the number of types of combination of the 28 duads 12,13,...78, taken 1,2,3,...27 together, a question presenting itself in geometry in regard to the bitangents of a quartic curve (*see* "Salmon's Higher Plane Curves," Ed. 2 (1873), pp. 222 et seq.); the numbers, so far as they have been obtained, are

$$\text{No. of types}=\frac{1,2,3,\ 4\ \ldots\ldots 24,25,26,27}{1,2,5,11\ldots\ldots 11,\ 5,\ 2,\ 1}$$

It might be interesting to complete the series; and more generally to determine the number of the types of combination of the $\frac{1}{2}\,n\,(n-1)$ duads of n letters.

"On Ternary Differential Equations," by **Robert Rawson**, Esq., Hon. Member of the Society.

1. The following observations have been suggested by reading the interesting communication, to the ordinary meeting of November 28th, 1876, "On Ternary Differential Equations," by Sir James Cockle.

With a view of comprehending fully the nature and importance of the step advanced by Sir James Cockle in the communication above referred to, it will be necessary to state briefly the exact state of our present knowledge of this subject, as given by Boole and other eminent writers on Differential Equations.

The substance of this knowledge may be given in two propositions, as follows:—

First.—That the differential equation

$$Pdx + Qdy + Rdz = 0 \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

where P,Q,R are given functions of x,y,z, has a single solution, or in other words, there is a single relation between the variables x,y,z which will satisfy it, providing the following conditional equation obtains

$$P\left(\frac{dQ}{dz}-\frac{dR}{dy}\right)+Q\left(\frac{dR}{dx}-\frac{dP}{dz}\right)+R\left(\frac{dP}{dy}-\frac{dQ}{dx}\right)=\Box=0 \;\ldots\; (2)$$

$\Box$ is called, by Sir James Cockle, the discriminoid.

Second.—If the conditional equation (2) does not obtain by virtue of the given functions P,Q,R, then the equation (1) has a *dual solution*, or in other words, there are two relations between the variables x,y,z which will satisfy it.

2. The first of these propositions was known to Newton and his contemporaries, who, however, regarded all those differential equations which failed to satisfy (2) as absurd and meaningless; Monge supplied the *dual solution*, and thereby removed the reproach of absurdity and meaningless from those differential equations which failed to satisfy the conditional equation (2).

3. Both of the above propositions are considerably circumscribed by a *third proposition* discovered by Sir James Cockle, viz.: equation (1) may fail to satisfy (2) and still be satisfied by a single relation between the variables of which it is composed.

The importance of this proposition can hardly be exaggerated, especially in reference to the doctrine of curved surfaces, &c., still, it might be hazardous, in the present active state of mathematical investigation both here and on the continent of Europe, to say it is new. It is not, however, referred to by Boole, De Morgan, M. L'abbé Moigno, the ablest writers on differential equations.

4. Sir James Cockle has shown, also, that when equation

(1) fails to satisfy the conditional equation (2), and still possesses a single solution, this solution is gathered from the equation $\square = 0$.

The solution here alluded to is not inaptly called the *discriminoidal solution* by Sir James Cockle.

5. The following view of the subject is intended to further elucidate the proposition in art. 4, and also to show a reason for the existence of these *discriminoidal solutions.*

By a reference to the general primitive and its derived differential equation, it will not be difficult to perceive the manner by which the harmony of the conditional equation (2) is disturbed by the legitimate process of elimination between the primitive and its differential equation. Take, for example, a simple case:

$$\text{Let} \quad (y + x^2)z - y = 0 \ \ldots\ldots\ldots\ldots\ldots\ldots \ (3)$$

be the primitive. Differentiate in the ordinary way, then

$$(Z - 1)\frac{dy}{dx} + 2xz + (y + x^2)\frac{dz}{dx} = 0 \ \ldots\ldots\ldots\ldots \ (4)$$

I prefer writing differential coefficients to differentials. Now, equation (4) has evidently a single solution (3), and satisfies the conditional equation (2).

This is all right enough; but, now observe the effect produced on the discriminoid $\square$ by eliminating $2xz$ from equation (4) by means of its value as given by equation (3).

$$\text{From (3)} \quad 2xz = \frac{2xy}{y + x^2}$$

Substitute this value in equation (4) and it becomes

$$(z - 1)\frac{dy}{dx} + \frac{2xy}{y + x^2} + (y + x^2)\frac{dz}{dx} = 0 \ \ldots\ldots\ldots\ldots \ (5)$$

Equation (5) has still the single solution (3); but it now fails entirely to satisfy the conditional equation (2).

$$\text{The discriminoid} \quad \square = \frac{2x}{y + x^2}\left\{ (y + x^2)z - y \right\} \ \ldots\ldots \ \ldots\ldots \ (6)$$

The discriminoidal solution of equation (5) is, therefore, a factor of the discriminoid $\square$, in accordance with the proposition of Sir James Cockle, as stated in art. 4.

Let us take another very general equation, viz.:

$$\left(\frac{dw}{dy}z-\frac{dv}{dy}\right)\frac{dy}{dx}+\frac{v}{w}\,\frac{dw}{dx}-\frac{dv}{dx}+w\frac{dz}{dx}=0 \ldots\ldots\ldots\ldots\ldots \quad (7)$$

where w,v are functions of x,y only.

This equation does not satisfy the conditional equation (2), as the discriminoid $\square$ is readily found to be as follows:

$$\square=(wz-v)\left\{\frac{1}{w}\,\frac{dw}{dx}\cdot\frac{dw}{dy}-\frac{d^2w}{dxdy}\right\} \ldots\ldots\ldots\ldots\ldots\ldots \quad (8)$$

This equation vanishes only when one of the two factors of which it is composed vanishes.

The first factor, viz., $wz-v=0$, satisfies equation (7), therefore, it is the discriminoidal solution of (7).

In consequence of $wz-v=0$ being a very general primitive, it is inferred that equation (7) is a very general differential equation which admits of discriminoidal solution.

6. Boole, in page 285, has, by a reference to one of the dual equations, unnecessarily limited the values of the constants in the equation

$$y\frac{dy}{dx}+x+c\sqrt{1-\frac{x^2}{a^2}-\frac{y^2}{b^2}}\cdot\frac{dz}{dx}=0$$

so as to admit of a single solution.

This equation is satisfied by the single solution

$$c^2(z-c)+a^2(x^2+y^2)=a^4$$

When $a=b$, the quantity C is introduced by integration.

7. The elimination of an arbitrary constant between the primitive and its derived differential equation does not disturb the harmony of the conditional equation $\square=0$.

$$\text{Let } (\mathrm{C}-\mathrm{R}_1)(\mathrm{C}-\mathrm{R}_2) \text{ \&c.}=0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (9)$$

be a primitive where (C) is an arbitrary constant and $\mathrm{R}_1,\mathrm{R}_2$, &c., given functions of x,y,z.

Differentiate (9), then

$$\left\{\frac{d\mathrm{R}_1}{dy}(\mathrm{R}_2-\mathrm{C})-\frac{d\mathrm{R}_2}{dy}(\mathrm{R}_1-\mathrm{C})\right\}\frac{dy}{dx}+\left\{\frac{d\mathrm{R}_1}{dx}(\mathrm{R}_2-\mathrm{C})+\frac{d\mathrm{R}_2}{dx}(\mathrm{R}_1-\mathrm{C})\right\}$$

$$+\left\{\frac{d\mathrm{R}_1}{dz}(\mathrm{R}_2-\mathrm{C})+\frac{d\mathrm{R}_2}{dz}(\mathrm{R}_1-\mathrm{C})\right\}\frac{dz}{dx}=0 \ldots\ldots\ldots\ldots \quad (10)$$

This equation evidently satisfies the conditional equation (2).

Eliminate (C) from (10) by means of equation (9) and there results

$$(R_1 - R_2)^2 \&c. \left\{\frac{dR_1}{dy}\cdot\frac{dy}{dx} + \frac{dR_1}{dx} + \frac{dR_1 dz}{dz\, dx}\right\}\left\{\frac{dR_2 dy}{dy\, dx} + \frac{dR_2}{dx} + \frac{dR_2 dz}{dz\, dx}\right\} \&c. = 0 \ldots (11)$$

Now, each of the factors equated to zero evidently satisfies the conditional equation (2).

8. It is readily seen that (11) is satisfied by (9) the general primitive, and also by $R_1 - R_2 = 0$, an equation which is not included in the general primitive by giving to (C) any value independent of x,y,z. Therefore, $R_1 - R_2 = 0$ is a singular solution of (11).

The following simple rule derived from the above is of some importance in finding the singular solution of a differential equation from its general primitive.

Solve the general primitive algebraically with respect to the arbitrary constant, the roots being generally functions of x,y,z. *The condition of equal roots is a singular solution.*

Professor Cayley informs me that the above rule is well known at Cambridge. This may be so; still, I have never seen it in print, and it is not mentioned by Boole in his differential equations. It was discovered by me some fifteen years ago, when I communicatied it to my pupils and mathematical friends, amongst whom I may mention the names of Rev. Robert Harley, F.R.S., &c., and the late Mr. Parkis, senior wrangler of 1864, and Vice-Principal of the School of Naval Architecture at Kensington. It may be further stated that if a differential equation of two variables be solved algebraically with respect to $p\left(=\frac{dy}{dx}\right)$, the roots being functions of x,y, the condition of equal roots which satisfies the differential equation is a singular solution.

Professor **Osborne Reynolds** exhibited various forms of vortex motion in a large glass tank by means of colour, or bubbles of air, the vortex lines behind an oblique vane, the vortex ring behind a circular disc, the vortex rings caused by raindrops, and the vortex rings caused by a puff of water. The various ways in which these vortices move were also shown. But Professor Reynolds' object in showing these experiments was to illustrate the importance of the method of study rather than from the intrinsic importance of the results already obtained, which are not as yet sufficiently complete for publication.

"On the powerful Oxidising Action of Animal Charcoal upon Organic Matters as shown by the Analysis of the Drainage from a large heap of a Mixture of Night-soil and Animal Charcoal," by WILLIAM THOMSON, F.R.S., Edin.

I had occasion some time ago to examine a sample of the fluid which had drained from a large heap of several thousands of tons of a mixture of Night-soil and Animal Charcoal which had lain for about one year or more, covered over with clay and pitch to prevent the rain from washing it away. This heap was about 7 or 8 feet in height, and the drainage from the whole which could be collected, did not amount to more than an average of about 12 gallons in 24 hours. It exuded from the heap in minute streamlets which drained down its sides, and at no part of this immense collection of oxidising organic matter could the slightest unpleasant odour be detected. The liquid which drained away was mixed with a small amount of suspended matter, which however soon settled to the bottom, leaving a perfectly colourless solution which was quite free from smell, but possessed a strong saline taste, and when treated with hydrochloric, sulphuric, or other acid it produced a copious effervescence.

This liquid was submitted to a very careful analysis in the following manner.

1. 50 c.c. of the liquid was evaporated to dryness in a tared platinum capsule and heated in an air bath at 220° Fah. till it ceased to lose weight, and its weight then noted.

2. This residue was then heated for some time to redness to destroy and free it from any organic bodies and again weighed.

3. The remaining mineral matters were then submitted to analysis by separation of the different salts by means of alcohol of different strengths to corroborate the results of the more exhaustive analysis.

4. 5 cubic centimetres of the solution were mixed with 245c.c. of pure water, free from ammonia, distilled and the free ammonia determined by means of "nesslerizing" to shew the proportion of ammonia in combination with carbonic acid. Pure carbonate of soda solution and pure water were added to the liquid remaining in the retort and it was again distilled till the distillate ceased to shew any coloration by the nessler test, and the ammonia from this distillate estimated by nesslerizing. This operation would give the proportion of ammonia which, although in the analysis of potable water would be termed "free ammonia" would in this case be in combination with hydrochloric acid. More pure water and permanganate of potassium were added to the solution, again remaining in the retort, and the liquid again boiled and the distillate collected with the same precautions as before, and the ammonia contained in it estimated. This should give an approximate idea of the nitrogen which is contained in the water, not as ammonia, but presumably in combination in some organic compound.

5. 100 c.c. of the sample were heated with a little free sulphuric acid, and a standard solution of potassium permanganate added till the liquid ceased to decolorize it, to find the amount of oxygen required to oxidise any matters capable of oxidation by such means.

The following determinations were made in the original water:—

Chlorine.—This was estimated by standard nitrate of silver solution.

Sulphuric acid was determined by the usual gravimetric process.

Potash was weighed as potassium plato-chloride, the platinum being added after the sulphuric acid had been separated by baryta water, the baryta with milk of lime, and the lime with ammonium carbonate and a little oxalate.

Magnesia was determined and weighed as magnesium pyrophosphate.

The free carbonic dioxide and that in combination with ammonia were estimated together by boiling 200 c.c. of the water in a flask and passing the distillate through a refrigerating apparatus and directly into a solution of barium chloride and ammonia, the resulting precipitate of barium carbonate was washed, dried, gently ignited, and weighed.

The *total quantity* of carbonic acid in the water was determined by treating 50 c.c. of the sample with an ammonia solution of barium chloride, and drying the resulting precipitate of barium sulphate and carbonate, placing it in a carbonic acid apparatus, and expelling the carbonic dioxide by means of sulphuric acid, and determining its amount by loss.

I may say that each result herein given was repeated two, three, or more times, so as to leave little or no doubt as to its accuracy.

The following gives the results of this analysis:—

	Grains.		Grains per Gall.
1°. Solid matter left on evaporating 4000 grains of the sample to dryness and *heating* at 220° Fah. till the residue ceased to lose weight	111·972	=Total solid matter......	1959·510
2°. Weight of residue after prolonged heating to redness	111·898	=Loss on ignition........	1·295
	Grammes.		
3°. Loss by treating the precipitate of Barium Sulphate and Carbonate produced from 50 c.c. of the sample, with sulphuric acid and water in carbonic acid apparatus	0·2655	=Total Carbonic dioxide	371·700
4°. Weight of the gently-ignited precipitate of Barium Carbonate produced from the distillation of 200 c.c, of the sample	2·7612	=Carbonic dioxide, free, and in combination with Ammonia	215·995
5°. Ammonia (NH_3) obtained by distilling 5 c.c. of the sample with 245 c.c. of pure water and "nesslerizing"	·00575	=Ammonia in combination with Carbonic dioxide.............	80·500
6°. Ammonia (NH_3) by distilling the liquid left in the retort from 5° with sodium carbonate and "nesslerizing"	·00010	=Ammonia presumably in combination with Hydrochloric Acid ..	1·400
7°. Ammonia (NH_3) obtained by distilling the liquid left in the retort from 6° with Potassium permanganate and "nesslerizing"	·0000325	=Nitrogen, presumably in combination in some organic compound ..	0·374
Weight of ignited precipitate of Magnesium pyrophosphate from 50 c.c. of the sample	·0928	=Magnesium	27·964
Weight of dried precipitate of Potassium Plato-Chloride from 25 c.c. of the sample.................	1·2053	=Potassium	539·840
Weight of ignited precipitate of Barium Sulphate from 50 c.c. of the sample...................	1·3286	=Sulphuric Anhydride..	638·640
50 c.c. of the sample required 27·2 c.c. of standard Silver Nitrate solution (each c.c. of st. solution is precipitated by ·006306 grammes pure sodium chloride)=weight of sodium chloride	0·17152	=Chlorine	145·719
100 c.c. of the sample required 9 c.c. standard Potassium Permanganate solution. Each gramme of oxalic acid required 550 c.c. of the st. solution to oxidise it. =weight of oxalic acid	0·01636	=Oxygen required to oxidise matters existing in the sample........	1·454
		Nitrates and Nitrites..	Absent

The water contained a large proportion of sodium salts. This base however was not estimated directly, the excess of acids being calculated as having been combined with that base.

A series of experiments were made, as follows, to decide how these bases and acids were combined.

1. Saturated solutions of pure ammonium chloride and potassiūm sulphate were prepared and mixed together, an excess of the chloride in each experiment being used. Alcohol was then added, and the resulting precipitate of sulphate washed with alcohol till free from chlorides.

2. Saturated solutions of pure ammonium sulphate and potassium chloride were mixed together, and the resulting sulphate precipitates freed from chlorides as above mentioned. The sulphate in each of the two above-mentioned cases was found to be *potassium sulphate.*

3. The same experiments were made with sodium chloride and potassium sulphate on the one hand and sodium sulphate and potassium chloride on the other, and the resulting sulphate in both cases found to be *potassium sulphate.*

The same experiments were made with ammonium sulphate and sodium chloride on the one hand and ammonium chloride and sodium sulphate on the other. The resulting sulphate in both cases being *sodium sulphate.*

5. The same experiment was made with sodium carbonate and potassium sulphate on the one hand and sodium sulphate and potasssium carbonate on the other. The resulting sulphate in both cases being *potassium sulphate.*

Part of the magnesium carbonate being precipitated by boiling some of the liquid under examination left no doubt as to the form in which it existed in the solution.

From these data I have arranged the analysis of the sample of drainage liquid as follows:—

	Grains per Gallon.
Total solid matter	1959·510
Organic matter, combined water, &c.	1·295
Fixed saline matter	1958·215

The fixed saline matter is composed of

Magnesium carbonate	97·874
Sodium carbonate	251·598
Potassium sulphate	1202·559
Sodium sulphate	153·566
Sodium chloride	235·311
Nitrates and Nitrites	Absent.
Loss	17·307
	1958·215

The following gives the proportions of free carbonic dioxide and volatile saline matters:—

	Grains per Gallon.
Free carbonic dioxide	17·110
Ammonium sesqui-carbonate	279·380
Ammonium chloride	4·406
Nitrogen, presumably existing in combination in some organic compound	0·370
Oxygen required by potassium permanganate test	1·454

There are many remarkable points about this drainage water.

First. Although it comes directly filtering from what originally was most noxious organic matter, it is undoubtedly free from any of those substances, of which, albumen may be taken as a type.

Second. That all these organic matters have been practically completely decomposed by oxidation into carbonic acid, water, and ammonia, and the drainage remains charged with enormous quantities of these products. An idea of the quantity of carbonic dioxide present may be had by saying that 100 c.c. of the water contains 268·54c.c. of this gas when measured at 0°C. and under a pressure 760m.m. of mercury.

Third. That although the oxidation of the organic matter had been so complete, yet the water was free from any trace of nitrates or nitrites.

Fourth. That the water was free from any trace of lime or phosphoric acid, but contained a comparatively large proportion of magnesium carbonate, which was kept in solution by the ammonium salts and free carbonic acid; the presence of this magnesium salt would no doubt account for the absence of phosphoric acid.

Fifth. The sediment and solution are practically free from bacteria or other animalcula.

Sixth. That when the residue from a large proportion of the water is heated to redness it produces no charring nor smell.

It might be interesting here to compare a few of the results of this analysis with those from a water which I collected about the same time which drained during a heavy rain from decomposing animal matter, principally butchers' offal, which had not been treated with charcoal. It contained

DRAINAGE FROM ANIMAL MATTER WITHOUT CHARCOAL.

	Grains per Gallon.
Total solid matter left on evaporating to dryness and heating at 220° F. till it ceased to lose weight	272·335
Matter lost by prolonged heating to redness	118·475
Saline matter	153·860
Free ammonia	15·447
Chlorine	13·394
Oxygen required by potassium permanganate test	85·629

When the dry residue was heated to redness it emitted a very bad smell at first, and afterwards the smell of burning hair.

Microscopic examination showed abundance of animalcula swimming about in all directions.

The charcoal with which the night-soil had been mixed deserves some notice. It was that produced in the manufacture of prussiate of potash by the charring of animal refuse such as hoofs, hair, leather, woollen rags, &c., so that although it is really "animal charcoal," it differs very much from the substance usually known under that name, viz.

that obtained by heating bones to a red heat in closed vessels. It appears to have a powerful effect in absorbing and oxidising noxious gases probably greater than any other species of charcoal. I have read an interesting lecture given by Dr. Stenhouse of London, many years ago, which was kindly placed in my hands by our worthy President Mr. Binney. He made some experiments to decide the value of different charcoals, and came to the conclusion that animal, more properly speaking bone charcoal, was best adapted for absorbing colour from liquids, but wood charcoal was best adapted for absorbing noxious gases; he however draws a distinct line between the capabilities of a charcoal to simply absorb on the one hand, and to absorb and then oxidise or decompose noxious gases on the other, but he does not mention the results of any experiments made with the charcoal under consideration. The following analysis of this substance was made and given to me by Mr. Spiegel of Oldham.

	Per Cent.	
Water	30·51	
Organic and volatile matters	4·52	*
Carbon	22·79	
Sand and insoluble matter	16·30	
Oxide of iron and alumina	12·66	
Lime	2·11	
Magnesia	·50	
Sulphuric acid	5·33	
Potash	3·117	
Soda	·759	
Ferrocyanic acid	·315†	
Traces of phosphoric acid, carbonic acid, and loss	1·059	
	100·000	

In conclusion I have to thank Mr. Ralph Clifton, of Stockport, for the care and manipulative skill which he has shewn in making the analysis of the sample of drainage water.

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* Containing nitrogen=ammonia .... 1·035

† Existing as Prussian blue

"A Plea for the word 'Anglo-Saxon,'" by **ROOKE PENNINGTON**, LL.B., F.G.S.

There has of late years arisen a class of historians who have taken under their especial protection the history of this country from the year of our Lord 450. Amongst other changes which it is sought to effect in the story of England is the abolition of the word "Anglo-Saxon." Without in any way attempting generally to review the work of these writers, I think it is not inexpedient to urge a few reasons somewhat crudely put together, for the retention of this particular term. A change which disturbs the historical and literary nomenclature of centuries, which would necessitate the annotation of our greatest authorities on our language and constitution is not an unimportant matter. The reasons put forward for the change appear to be two, *first*, that the word is an incorrect one, *secondly*, that it is a misleading one. I will consider these reasons shortly, before dealing with the reasons why the word should be retained.

First as to its incorrectness. From A.D. 450 to A.D. 550 Britain was invaded by bands of Saxons, Angles, and Jutes. From A.D. 830 to A.D. 1040 it was invaded by bands of Danes. All these peoples settled in various parts of Britain. The Saxons, Angles, and Jutes sometimes called themselves by these names, sometimes called themselves Angles or (modernized) English. The Danes at first called themselves Danes, but ultimately were confounded in name with the other tribes. Now Anglo-Saxon is an incorrect name for the combination of these four peoples, because it is manifestly imperfect. But it is a good word, as words go. There is a limit to compound words (except perhaps in organic chemistry). It is impossible to express in the name of a people all the races which go to compose it. If we were to reject this word because inaccurate, and consistently went on to weed our language, what a host of useful words would disappear. And although these peoples did sometimes get called English, I think we shall see that this is no good reason why we should do so. If now for the first time we were writing an account of our history and reviewing the past of our language, it might perhaps be better to begin by calling these peoples the English. But unfortunately we cannot do this.

The second objection advanced against the term is that it is a misleading one and that it obscures the fact that the great parent stocks of the English people were these North German tribes. But does it? Who is misled? That the English of to-day are a mixed race, but mainly springing from those who used to be called the Anglo-Saxons, is about as well recognized a fact and as widely known a fact as any historical fact can be. I was greatly astonished to find that Mr. E. A. Freeman, speaking in Manchester at the prize-giving of the Mechanics' Institutes, uttered words of wholesale condemnation of school histories as usually misstating that Julius Cæsar encountered English in Britain and that he fought with our ancestors. It is partly true that he did fight with our ancestors, but I venture to say that not one of the students he addressed ever saw either this or the other statement in his school-books put as Mr. Freeman put it. Out of curiosity, I examined twenty-three school histories, price one penny upwards, and in every one the real truth was as correctly stated as Mr. Freeman could state it. In particular, I consulted two friends of my early boyhood—"Pleasant Pages" and Charles Dickens's Child's History of England. The only difference between them and Mr. Freeman was that they put the matter as an ordinary well-known part of history never doubted by anybody, instead of the recent and important discovery of a new historical school.

Incidentally, I may remind you that the word "Anglo-Saxon" is a very old one. It is not certain whether the first kingly title implying a common rule over the invaders was "rex Anglorum" or "rex Anglorum Saxonorum." But whether the latter designation was used before Egbert or not, Alfred was certainly styled "Angul-Saxonum Rex."

I am not pretending to write an exhaustive essay on the Anglo-Saxons, but as I have stated the objections, let me now produce a few reasons, cogent, I think them, for the retention of the term. First as to race. By Anglo-Saxons is meant the three tribes who invaded England from A.D. 450 to A.D. 550. Then came the Danes, then the Normans. There is wanted some historical terminology which will distinguish the period before the fusion of the blood of these three sets of invaders from the period after the fusion. Has not the word "English" been generally used to indicate the period from the time of Canute downwards? Even then there was only partial fusion of Angle and Saxon and Dane, and the Norman was only beginning to arrive. In the nature of things historical, lines of demarcation must be rough. But better rough than none at all, And the word "English" being by general understanding appropriated to one time, and "Anglo-Saxon" to another, why mystify us by abolishing one term. The German called himself the Angle, the conquered Celt called him the Saxon. German and Celt are now one, began to be one when Christianity and Danish conquest welded the tribes into a nation, and when incessant intercine wars of petty chiefs ceased in the struggle for a kingdom. What then can be more appropriate than that Anglo-Saxon should be retained to designate the earlier period before the invader had fused with the older people into our modern English?

Now as to language. In literature classification is more accurate, rules are more definite, than in history. So we find the changes in our language more clearly marked out than in our ancestry. True, the lines do coincide with those of history, literature only follows in the wake of events. In history "English" is of earlier date than it is in letters. First reaching down to the Norman conquest, comes the period of Anglo-Saxon with its gradually fusing Angle and Saxon dialects, its Celtic sprinkling of words, and its slowly dying inflections. It has been said that the notion of the Celtic infusion is a mistake, that the savage German swept all before him, killed all he could in battle, and murdered all the conquered after; nay, that his glut for blood was such that he was not content till his vengeance had exterminated the very cows. But our language disproves this. Whence came the Celtic terms of servility and abuse, if not from the Celtic bondsman? Whence are the Celtic names for hill and valley and stream, if the name givers had no opportunity of teaching their conquerors their rude geography? Besides, it is forgotten that when the fighting ceased all Cornwall, all Cumbria, and much of the Penine hills were still Celtic. When the fighting ceased, fusion began.

It has also been said that because English speech is a lineal descendant of Anglo-Saxon, that should be called English too. Do we call Latin Italian, or Greek Romaic? And why should we style a language English which is as hard for us to learn or to understand as is modern German?

Following the Anglo-Saxon period, that from the Conquest to Henry III. is known as Semi-Saxon. The English period commences with the celebrated proclamation of this king, and is divided into Old English, Middle English, and Modern English, the latter period dating from the Reformation. Meanwhile a complete change had taken place in the old Anglo-Saxon. Not of course that there had been actual amalgamation with Norman French or any other tongue. The grammar remained and still remains Anglo-Saxon. A syntactical combination of two languages is almost an impossibility. But Norman French, following the like influence of Danish, accelerated the complete disuse of almost all the Anglo-Saxon inflections—in fact changed the inflectional Anglo-Saxon for the prepositional English, and Norman words were transplanted and took root. The first English writer of commanding genius, Chaucer, established in our language a host of Norman French words, which enriched the poverty-stricken vocabulary of the Anglo-Saxon, and made (with the Celtic words) the English of his day a composite tongue. Of how heterogeneous a nature is our dictionary of to-day I need not say.

The historical and literary meanings of the words Anglo-Saxon and English which I have thus sketched are in the main those which have been established by the profoundest authorities. And for the sake of clearing up an historical misapprehension which does not exist, we are to obscure the work of Sharon, Turner and Kemble, of Palgrave and Lappenberg, of Bosworth and of Latham, and to proclaim that all the great English historians who have ever written must now be re-issued with explanatory notes correcting their common blunder. Is there any disease demanding such a remedy? We are of mixed race, speaking a mixed language. We call both race and language English. It will introduce far greater confusion to extend this term to a time before the mingling of blood and vocabulary had well begun than ever the old distinctions have caused. I must ask pardon for having had to go over so much elementary ground to prove this, but we have popular writers now-a-days who write as though the great students and authorities of the past had never existed, and there is perhaps no harm done in recalling to our recollection the results they laboriously arrived at, results which, perhaps, are still worthy of some little attention.

The following paper was read at the meeting held January 23rd, 1877:—

"Results of the Monthly Observations of the Magnetic Dip, Horizontal Force, and Declination, made at the Magnetic Observatory of the Owens College, from January, 1874, to December, 1876, inclusive," by Professor THOMAS H. CORE, M.A. Communicated by Professor BALFOUR STEWART, LL.D., F.R.S.

This paper, containing the results of 3 years magnetic observations, is in continuation of a paper by Professor B. Stewart in No. 10, Vol. xiii. of the Society's Proceedings, p. 111, containing the results for 1873 of the Observations for Dip and Horizontal Force. For convenience of reference, Dr. Stewart's numbers are incorporated in Tables I. and II.

The observations were made by Mr. Kingdon and myself and latterly by Mr. Poynting, and the two days of the month on which they were made were always as near the end of the third week or beginning of the fourth week as possible. The instruments and methods of observation were the same as before. For the declination, which was not before observed, in addition to the Unifilar by Elliott Bros., a transit Theodolite by Troughton & Simms was used to determine the azimuth of a fixed southern mark—the highest point of the steeple of Heaton Mersey Church. A mean of several transit observations of the sun gave the south azimuth of the steeple 14° 0′ 25″.

TABLE I.

MAGNETIC DIP.

	1873.	1874.	1875.	1876.	Mean of 4 Years.
January.......	69° 19′·5	69° 19′·5	69° 13′·8	69° 10′·7	69° 15·62
February......	14′·6	14·7	12·3	13·0	13·65
March	17·6	12·4	13·2	*12·7	13·97
April	18·9	16·8	12·4	*14·8	15·72
May	17·7	17·1	13·6	*12·5	15·22
June	16·1	15·3	15·4	16·0	15·70
	69° 17′·40	15·80	13·45	13·28	69° 14′·98

	1873.	1874.	1875.	1876.	Mean of 4 Years.
July...........	69° 14′·6	69° 14′·8	69° 10′·8	69° 9′·9	69° 12′·52
August	20·2	14·4	13·2	12·7	15·12
September...	23·8	*12·8	14·4	12·0	15·75
October......	16·8	14·4	15·3	13·3	14·95
November...	14·4	*14·5	14·1	8·2	12·80
December...	18·3	*13·6	15·0	11·6	14·62
	69° 18′·02	14·08	13·80	11·28	69° 14′·30
Yearly Means	69° 17′·71	69° 14′·94	69° 13′·62	69° 12′·28	69° 14′·64

The six numbers marked with an asterisk result from observations of one needle only, but as the mean annual difference of the readings for the two needles for these years differed by less than the tenth of a minute, no correction has been applied to them.

The annual and semi-annual inequalities of the dip cannot be accurately deduced from four years' observations, but an approximation to the former is exhibited in the following table.

		Annual Decrease.
Mean Dip in 1873	69° 17′·71	2′·77
„ „ in 1874	69 14·94	1′·32
„ „ in 1875	69 13·62	1′·34
„ „ in 1876	69 12·28	
Mean	69° 14′·64	1′·81

The mean annual decrease in the dip of 1·81 minutes corresponds very well with that of 2′·02 as found from the Kew observations from 1857 to 1874 inclusive, and 1′·90 from the Stonyhurst observations from 1871 to 1874 inclusive.

TABLE II.

HORIZONTAL FORCE.

	1873.	1874.	1875.	1876.	Mean of 4 Years.
January	....	3·694	3·699	3·700	3·6977
February	....	3·691	3·693	3·704	3·6960
March	3·693	3·692	3·709	3·695	3·6972
April	....	3·691	3·703	3·702	3·6987
May	3·686	3·705	3·693	3·701	3·6962
June	3·684	3·700	3·697	3·708	3·6972
	3·6877	3·6955	3·6990	3·7017	3·6971

	1873.	1874.	1875.	1876.	Mean of 4 Years.
July	3·704	3·696	3·693	3·699	3·6980
August	3·704	3·709	3·696	3·699	3·7020
September	3·715	3·691	3·697	3·705	3·7020
October	3·661	3·691	3·696	3·701	3·6872
November	....	3·689	3·697	3·703	3·6963
December	3·686	3·691	3·707	3·702	3·6940
	3·6940	3·6945	3·6977	3·7015	3·6966
	3·6916	3·6950	3·6983	3·7016	3·6969

The values of the horizontal force, corresponding to the middle of each of the four years, and the annual increase for each year are as follows:—

		Annual Increase.
Mean horizontal force for 1873	3·6916	·0034
,, ,, ,, 1874	3·6950	·0033
,, ,, ,, 1875	3·6983	·0033
,, ,, ,, 1876	3·7016	
	3·6969	·0033

If we multiply the mean horizontal force for each year successively by the secant, and the tangent, of the mean dip, for the same year we get the following values of the Total force and the Vertical force for the four years.

	Mean Total Force.	Annual Diminution.	Mean Vertical Force.	Annual Diminution.
1873...............	10·4414	·0126	9·7670	·0147
1874...............	10·4288	·0013	9·7523	·0025
1875...............	10·4275	·0011	9·7498	·0029
1876...............	10·4264		9·7469	
Means............	10·4360	·0050	9·7540	·0067

TABLE III.

DECLINATION.

	1874.	1875.	1876.	Mean.
January	......	......	20° 47′ 20″	20° 47′ 20″
February	......	......	20 43 30	20 48 30
March	20° 51′ 10″	20° 49′ 0″	20 35 20	20 45 10
April	20 48 40	20 46 0	20 32 40	20 42 27
May	20 24 30	20 41 50	20 36 0	20 34 7
June...............	20 35 30	20 40 40	20 33 0	20 36 23
Mean	20 39 58	20 44 23	20 37 58	20 40 22
July	20 38 20	20 36 0	20 33 50	20 36 8
August	20 31 0	20 29 10	20 36 40	20 32 17
September	20 48 40	20 32 30	20 33 30	20 38 13
October	20 52 40	20 36 40	20 27 40	20 39 0
November	20 43 50	20 43 50	20 41 0	20 42 53
December	20 58 30	20 39 0	20 42 20	20 46 37
Mean	20 45 30	20 36 12	20 35 50	20 39 10
Annual Mean...	20 43 17	20 39 28	20 36 54	20 39 42

The values of the declination in the above table are not to be depended on to within two or possibly three minutes, as (1) the observation for the torsion of the suspending thread was not always made, (2) the hour of observation not being always the same, the results are affected by the diurnal variation, and (3) the results must also be affected by irregular disturbances.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 4th, 1876.

Charles Bailey, Esq., in the Chair.

"Notes on a Botanical Excurison in the Aberdeenshire Highlands in July, 1876," by Mr. Thomas Rogers.

The author started in company with Messrs. J. Nield and Ashton and John Whitehead, on July 14th, and commenced ascent of Ben Mac D'hui, from Aviemore the following day. On the way in brief words were noticed *Trientalis Europea* and the rare Musci, *Orthotrichum speciosum* and *O. leiocarpum*. Crossing the river Spey, they entered the woods of Rothiemurchus—in which was situated a swamp where was gathered Sphagnum laricinum—a plant almost new to Britain. Up higher grew Carex pauciflora; and many of the little rills hereabouts were much infested with a beautiful algæ (Batrachospermum moniliforme). Still moving upwards for about two miles, *Cornus suecica* and *Vaccinium uliginosum* were found, and the rarest of the six British Lycopodia (L. annotinum). The path now began to get rugged and barren, *Junus trifidus* being the principal plant. Near the top of the watershed among moss covered stones were found *Veronica alpina,* the rare

Saxifraga rivularis, *Epilobium anagallidifolium*, Arabis petræa, &c.

The party now descended into the valley of the Dee, where, having extemporized a heather bed, sheltered by large stones, they passed the night. The following morning they started early to ascend Ben Mac Dhui, and in two or three hours, after very hard walking over huge boulders and broken rocks, reached the sloping and gravelly top which led them gently upwards to the summit. Here *Luzula arcuata* was the principal plant; mingled with stunted specimens of *Luzula spicata*, *Carex rigida*, and *silene acaulis*. After resting a short time, they proceeded over a gently sloping gravelly headland towards Cairngorm, and ere long a change came over the character of the scene, for numerous little rills of water made their appearance, fresh from the melting snow, each boulder stone being covered with mosses and jungermanniæ of all shades of colours from light green to dark purple. Among others the glaucous green tufts of *cephalozia albescens* and *anthelia julacea*, the large purple patches of scapania uliginosa and Nardia compressa, the rich brown beds of *Polytrichium sexangulare* and dark green and dull purple of *Bryum Duvalii*, and black *Andrœa nivalis* were especially remarkable. The snow field was now crossed, and a stream of water appeared sufficiently large to debar the party crossing it with impunity; the course of this stream was pursued, and the shelter stones near Loch Avon discovered, where they passed another night. The next morning they started for Braemar by Glen Derry. Near the Corrie of Etichan were found Hieracium alpinum, Tofieldia palustris, Tetraplodon mnioïdes, &c.

On Wednesday (July 19) Glen Callater was visited. The principal plant noticed being a curious variety of Helianthemum vulgare, the base of the petals being blotched with dark orange spots. Sphagnum subsecundum, Salix lanata,

S. lapponum, S. reticulata, S. herbacea, and S. arbuscula, Veronica alpina and V. saxatilis, Carex atrata, Pyrola secunda, Zygodon lapponicus, Grimmia torquata and G. funalis and Sphagnum strictum were also noticeable.

The next two days were spent in a search for Linnea borealis, found on Friday 21st growing in great plenty in fir woods near the stream called the Carr Linn. Here also were noticed Sphagnum rubellum, Sphagnum acutifolium var. fuscum in fruit—Epilobium alsinifolium, Pyrola media, Meum athamanticum, &c.

Ordinary Meeting, February 20th, 1877.

Rev. WILLIAM GASKELL, M.A., in the Chair.

"Note on a Curious Allusion of a Writer of the 17th Century to a supposed Property of the Magnetic Needle, since verified in the Invention of Telegraphy," by **HARRY GRIMSHAW**, F.C.S.

Some little time ago my friend, the Rev. G. W. Reynolds, M.A., of Cheetham Hill, directed my attention to a paragraph in an old volume in his possession of some 250 years of age, which struck me as a peculiarly interesting one; so much so in fact, that I have taken the liberty of bringing it before the notice of this Society.

The work in question is entitled, "An Apologie of the Power and Providence of God in the Government of the World, or An Examination and Censure of the Common Error touching Nature's perpetual and Universal Decay, Divided into Four Books." The author is one "G. H.," D.D. (Doctor of Divinity), and the work is printed at Oxford by John Litchfield and William Turner, "Printers to the famous University." Anno Domini 1627, being therefore exactly 250 years old.

The third book of the four into which the work is divided treats of "The pretended decay of mankind in regard and duration, of strength and stature, of arts and wits." The tenth chapter of this third book is said to be "Touching diverse artificiall workes and usefull inventions, at leastwise matchable with those of the ancients, namely and chiefly the invention of Printing, Gunnes, and the Sea-Card or Mariners Compasse." This tenth chapter again, for such is the orderly division of the subjects, is sub-divided into four sections, and the fourth of these is headed "Of the use and invention of the Mariners Compasse or sea-card, as also of another excellent invention sayd to be lately found out upon the Load-stone, together with the conclusion of this comparison touching Arts and Wits, with a saying of Bodius, and another very notable one of Lactantius."

It is in the account of this "excellent invention sayd to be lately found out upon the loadstone" that the curious prevision or dream, so to speak, of the application of electricity as a means of communication occurs, and there is small wonder that the old philosopher called it as he does further on, "an excellent and secret conclusion upon the stone," for, whilst perusing his description, one can hardly imagine that the writer has not in his mind's eye one of our most modern telegraphic instruments. I quote the paragraph in its entirety:—

"Another excellent and secret conclusion upon this stone, pretended to be found out in these latter times, is, that by touching two needles with the same stone, they being severally set so as they may turne upon two round tables, having on their borders, the *Alphabet* within circlewise, if two friends agreeing upon the time, the one in Paris, the other in London (having each of them their table thus equally fitted) be disposed upon certayne dayes and at certaine houres to conferre, it is to bee done by turning the needle in one of the tables to the *Alphabet*, and the other, by *Sympathie* will turn itself in the same manner in the other table though never so farre distant: which conclusion if infallibly true, may likewise proove of good and great consequence; howsoever I will set it down as I find it described by *Famianus Strada* in imitation of the stile and vaine of *Lucretius*."

Magnesi genus est lapidis mirabile, &c., &c.

Then follows the extract in Latin, with the English translation in verse attached.

It will be acknowledged by anyone familiar with the instrument, that the dial telegraph of Cooke and Wheatstone, invented subsequently to their first upright needle form, most curiously carries out the ideal description of this old author, and it will be seen that the date at which his work is written was nearly 200 years prior to the first attempt made to communicate at a distance by means of magnetic needles.

The following dates may be incidentally mentioned on this point. First, in 1733, Du Fay succeeded in transmitting the electricity from an excited glass tube to the distance of 1256 feet along a wet packthread. Then in 1748 Franklin set fire to spirits of wine by means of a current of electricity passed through the water of the Shuylkill river. Neither of these experiments were however undertaken with any idea of transmitting signals, the first attempts of this kind being perhaps those of Lesage and of Lomond, in 1774 and 1787 respectively, who used the divergence of pith balls to indicate letters. These are all of course experiments with statical electricity, and we have to come down to Volta with his discovery of the Voltaic Pile in 1800 for the means of applying dynamical electricity for the purpose in question. Then through Oersted's discovery of the action of electrical currents on the magnetic needle, in 1819, and Ampère's suggestion, in 1820, that this action should be used in signalling, we come to Cooke and Wheatstone's first patent for a telegraphic instrument in 1837; bringing us through a journey of just about 200 years in order to arrive at our practical realisation of this vision of the old writer.

"On the action of Water and dilute Saline solutions upon Lead." Part iii. By M. M. PATTISON MUIR, F.R.S.E., Assistant Lecturer on Chemistry, the Owens College.

1. In the second part of these researches (Proc. Manch. Lit. and Phil. Soc., xvi. 1) I shewed that there is generally an increase in the solvent action of water and dilute saline solutions upon lead when the experiments are carried out in beakers covered with porous paper; also that exposure of large surfaces of liquid to the surrounding atmosphere generally tends to increase solvent action. Further my experiments led me to conclude that the solvent action tends to attain a maximum when the volume of liquid is large in proportion to the surface of lead exposed. I also concluded that under favourable conditions the quantity of lead dissolved increases in an increasing ratio with the time during which the action is allowed to proceed. (Compare Par. 6). Finally, I expressed my belief that the purity of the lead upon which the various liquids are allowed to act, very materially conditions the solvent action of those liquids.

In the present communication I propose to bring forward further experimental evidence as regards some of these points, and also to touch upon certain circumstances conditioning the solvent action of dilute saline solutions upon lead which have not as yet been studied.

2. And the first point which demands attention is one which I merely mentioned in my second paper, viz., Does the relation between surface of lead exposed and total quantity of liquid influence the solvent action? In the following experiments the surface of lead exposed was maintained constant, the strength of the saline solution was also unchanged, but the total quantity of liquid was increased. Otherwise the experiments were carried out under exactly similar conditions.

Table I.

Experiments carried out in Beakers half-filled with liquid (500cb.c. in each) and covered with unsized paper: diameter of mouth of Beaker=11·5c.m.

Salt.	Grams per litre.	Quantity of liquid.	Lead exposed.	Lead dissolved (Mgms.) per 500cb.c. of liquid.	
		cb.c.	sq. cm.	11 days.	21 days.
Ammonium Nitrate......	0·20	250	50	1·8	1·5
„ „	0·20	500	50	2·5	2·5
„ „	0·20	750	50	2·5	2·5
„ „	0·20	1000	50	2·5	1·8
Potassium Nitrate......	0·20	250	50	2 5	1·8
„ „	0·20	500	50	1·5	1·8
„ „	0·20	750	50	1·3	1·3
„ „	0·20	1000	50	1·0	1·0
Calcium Chloride	0·20	500	50	2·5	3·5
„ „	0·20	1500	50	1·3	2·0

Sometimes a slight increase is noticed in the quantity of lead dissolved by equal quantities of solutions, when the total volume of liquid is increased, on the other hand a slight decrease is sometimes noticed. I think that so far as these experiments go, we may conclude that a very large volume of liquid dissolves no more lead than a relatively small volume, when caused to act on the same amount of metallic surface.

3. The second question calling for an answer is—Does the quantity of lead dissolved increase with increase of time, or is there a maximum reached after which no further increase is noticeable?

Various experiments were carried out. Pieces of clean lead were immersed in different saline solutions for three months, and were then subjected to the action of various liquids. Comparable experiments, omitting the previous lengthened immersion in saline liquids, were also carried out. Special experiments in reference to Potassium Carbonate are detailed, inasmuch as in my last paper I had provisionally concluded that in the presence of this salt, the solvent action of water upon lead soon—comparatively speaking—reaches a maximum.

Fresh light is also thrown upon the question now under consideration by the results of experiments having for their special object a comparison of the solvent action of various liquids upon different samples of lead. (Par. 10.)

TABLE II.

LEAD IMMERSED IN STRONG SOLUTIONS OF THE UNDERMENTIONED SALTS FOR THREE MONTHS: THEN WASHED WITH A LITTLE DISTILLED WATER, AND IMMERSED IN VARIOUS SOLUTIONS.

EXPERIMENTS CARRIED OUT IN BEAKERS HALF-FILLED WITH WATER (500CB.C. IN EACH) AND COVERED WITH UNSIZED PAPER: DIAMETER OF MOUTH OF BEAKER=11·5C.M.

	Grams per litre.	Surface of Lead exposed in sq. cm.	Total Lead, in Mgms., in solution after			
Then in			6 days.	15 days.	38 days.	62 days.
A.—*Lead immersed in strong solution of Potassium Carbonate.*						
Potassium Carbonate	0·20	50	trace	trace	trace	trace
„ Nitrate	0·20	50	0·4	1·0	1·0	1·20
Ammonium „	0·20	50	1·5	8·5	2·0	1·00
B.—*Lead immersed in strong solution of Potassium Nitrate.*						
Then in						
Potassium Carbonate	0·20	50	0·20	0·20	0·20	0·30
„ Nitrate	0·20	50	0 30	0·30	0·20	0·30
Ammonium „	0·20	50	0·50	0·50	0·30	0·70
C.—*Lead immersed in strong solution of Calcium Chloride.*						
Then in						
Potassium Carbonate	0·20	50	0·20	0·20	0·20	0·50
„ Nitrate	0·20	50	1·20	0·50	0·40	0·50
Ammonium „	0·20	50	2·50	0·80	0·60	0·80
D.—*Lead immersed in strong solution of Ammonium Sulphate.*						
Then in						
Potassium Carbonate	0·20	50	0·30	0·50	0·40	0·50
„ Nitrate	0·20	50	3·00	8·00	1·90	1·80
Ammonium „	0·20	50	5·00	1·30	0·30	1·00

4. In order to compare these numbers with those obtained by submitting the same amount of surface of the same specimen of lead to the action of distilled water containing equal quantities of the same salts during the same intervals of time, I subjoin the following determinations taken from a paper already read before this Society. (xvi., p. 3.) The experiments were carried out under exactly similar conditions.

TABLE III.

Salt.	Grams per litre.	Surface of Lead in sq. c.m.	Total Lead in solution, in Mgms., after 18 days.
Potassium Carbonate...	0·20	50	0.3
„ Nitrate	0·20	50	2·4
Ammonium „	0·20	50	3·8

In these experiments the lead had *not* been previously immersed in saline solutions.

5. The following experiments have especial reference to the action of time in determining the quantity of lead dissolved by a water containing Potassium Carbonate in solution.

TABLE IV.

Experiments in Beakers, Corked Flasks, and Basins, details of measurements, &c., as in last Paper (xvi. p. 3). In each case 500c.c. of liquid containing 0·20 grams of Potassium Carbonate per litre. 50 sq. cm. of surface of lead exposed.

	Total Lead, in Mgms., in solution after 8 days.	10 days.	12 days.	20 days.
Flask...............	0·20	0·20	0·20	0·20
Beaker	0·30	0·30	0.30	0·30
Basin..............	0·40	0·40	0·40	0·40

6. The results of these experiments are rather peculiar: they certainly shew that the action upon lead of all the saline liquids with which I have experimented is an exceedingly complicated one, and that it is very difficult to separate one set of conditioning circumstances from all others.

In the case of Potassium Carbonate a maximum was reached so soon as at the end of eight days: after this time the quantity of lead in solution did not increase. If however the lead have been previously immersed in a solution of either Potassium Nitrate, Calcium Chloride, or Ammonium Sulphate, then the solvent action of the Potassium Carbonate Solution continues to increase (at least throughout so long a period as 62 days) although but to a very slight extent. Generally I conclude that the solvent action of saline solutions continues throughout very lengthened periods; certainly that it does so if the liquid be removed, and fresh liquid, holding the same or other salts in solution, be put in its place. In some cases however a maximum point appears to be reached after the expiry of 14 days or so, after which, if the liquid be undisturbed, little or no further solvent action occurs.

7. In some of these experiments a remarkable result is obtained, viz.: the quantity of lead in solution is found to *decrease* after a certain point has been reached. In order to determine whether the same result could be obtained when the solvent liquids were not exposed to the air, the following experiments were carried out in corked flasks each containing 500 cb.c. of water, the surface of lead exposed amounting as before to 50 sq. cm.

Table V.

Salt.	Grams per litre.	Total Lead, in Mgms., dissolved after 15 days.	32 days.	38 days.
Potassium Carbonate...	0·20	0·40	0·30	0·30
,, Nitrate......	0·20	0·90	0.70	0·60
Ammonium ,, ...	0·20	1·50	1·00	0·80

8. Here again we have a slight decrease in the solvent action of Solutions of Potassium and Ammonium Nitrates upon lead taking place after the expiry of lengthened periods. The lead would appear to be precipitated from solution after a time. A remarkable instance of such precipitation will be detailed in a future paragraph.

9. I must now consider the question of the purity of the lead itself as influencing the quantities of this metal dissolved by various liquids.

The experiments were carried out in flasks and beakers.

The sample of "pure" lead was found to contain very small traces of Manganese, Iron, and Zinc: the "commercial" sample, No. 1, contained small quantities of Antimony and Tin, very little Iron, no Manganese, Copper, or Bismuth, but traces of Aluminium: the impurities in No. 2 sample were the same as in No. 1, small quantities of copper being also present. The results are very startling. With No. 1 sample of commercial lead, less lead was dissolved in almost every case than with "pure" lead; with No. 2 sample, whose chemical composition appears almost the same as that of No. 1, *very much larger* quantities were dissolved. I at once supposed that the fact noticed by some previous experimenters, viz., that mechanical treatment of lead alters its power of withstanding the action of solvents, must have materially influenced the results. That mechanical treatment does alter lead to an astonishing degree, so far as the action upon it of saline solution is concerned, will be made apparent from the experiments detailed in par. 12.

TABLE VI.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	"PURE" LEAD. Total Lead, in Mgms., dissolved after 6 days.	"PURE" LEAD. Total Lead, in Mgms., dissolved after 14 days.	COMMERCIAL LEAD NO. 1. Total Lead, in Mgms., dissolved after 6 days.	COMMERCIAL LEAD NO. 1. Total Lead, in Mgms., dissolved after 14 days.	COMMERCIAL LEAD NO. 2. Total Lead, in Mgms., dissolved after 6 days.
EXPERIMENTS IN CORK FLASKS, NEARLY FILLED WITH LIQUID (500CB.C.)							
Distilled Water	—	25	—	1·2	trace	trace	12·0
Potassium Nitrate.	0·20	25	0·9	1·2	0·3	0·4	5·0
Ammonium „	0·20	25	1·3	1·8	1·5	1·0	12·0
Distilled Water	—	50	0·7	0·9	0·5	0·5	16·0
Potassium Nitrate.	0·20	50	1·1	1·2	1·7	0·5	12·0
Ammonium „	0·20	50	1·4	1·5	7·0	7·0	7·0
EXPERIMENTS IN BEAKERS, 500CB.C. LIQUID IN EACH.							
Distilled Water	—	25	0·4	0·5	0.3	0·3	0· 5
Potassium Nitrate.	0·20	25	0·4	0·4	0·3	0·3	trace
Ammonium „	0·20	25	1·2	2·0	1·0	1·0	trace
Distilled Water	—	50	0·7	1·2	0·3	0·3	trace
Potassium Nitrate.	0·20	50	1·4	2·2	0·3	0·4	trace
Ammonium „	0·20	50	2·3	3·5	0·3	0·5	trace

10. These experiments appear to show that it is not so much the chemical purity of the lead, as the mechanical

treatment to which it has been subjected, which influences the solvent action of dilute saline solutions upon that lead. But the most remarkable feature in the numbers contained in Table VI. is the extraordinary falling off in the quantities of lead dissolved from No. 2 commercial sample when the experiments were carried out in beakers, as compared with the quantities dissolved in corked flasks. Taking, for instance, a solution of Ammonium Nitrate containing 0·20 grams. of the salt per litre of water, it is found that 500 cbc. of this liquid, acting upon a surface of 25sq. cm. of lead, dissolves 12·0 mgms. after 6 days' action, when the experiment is carried out in a corked flask nearly filled with liquid; but that the same quantity of the same solution acting on the same surface of the same lead is only able to bring a slight trace (*i.e.* less than 0.20 mgm.) of lead into solution, after the expiry of the same time, when the experiment is conducted in a beaker half filled with liquid and loosely covered with porous paper. The conclusion appears to be that the action of the air results in the formation of an insoluble lead salt. That this conclusion is the correct one appears from the following experiments. In the experiments with No. 2 sample of commercial lead very considerable precipitates formed on the lead itself and on the bottom of the vessels used. This was the case both in those experiments which were carried out in corked flasks and in those in beakers, but in the latter cases the precipitate generally appeared somewhat more bulky. On withdrawing a quantity of the clear liquid—holding lead in solution—from the flasks, and exposing it to the action of the air, a precipitate was in each case almost immediately formed. This precipitate increased in amount, until after a few hours a mere trace of lead could be detected in solution: the whole of the lead had been precipitated in an insoluble form. The corks were then withdrawn from the flasks, and the mouths loosely covered with porous paper: after 5 or 6 days the liquids contained only traces of lead in solution. From the liquids containing Ammonium Nitrate the lead was precipitated more slowly than from any of the others. The precipitate consisted of hydrated carbonate of lead $2PbCO_3, Pb(OH)_2$.

11. These results are apparently contradictory of those previously obtained, inasmuch as it has been already shewn that in most cases more lead is dissolved when the air has free access to the surface of the liquid, and that the larger the surface of liquid exposed the greater is the amount of solvent action. I believe that the explanation of the results is to be sought for in the mechanical state of the samples of lead experimented upon.

12. With regard to the influence of hammering and rolling upon lead, so far as concerns the power of the lead to withstand the action of dilute saline solutions, I have meanwhile only a few experiments to bring before the Society. I propose, however, to investigate this subject in a more systematic manner, and hope, at a future date, to record the results.

TABLE VII.

EFFECT OF HAMMERING AND ROLLING ON LEAD.

EXPERIMENTS IN BEAKERS HALF-FILLED WITH LIQUID (500CB.C. IN EACH) COVERED WITH UNSIZED PAPER.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	Nature of Lead.	Lead dissolved, in Mgms., after 11 days.	21 days.
Potassium Nitrate...	0·20 ...	50	...Thin Lead foil "pure."	1·50 ...	1·80
„ „ ...	0·20 ...	50	...4 or 5 sheets of same lead rolled together into compact piece	trace ... 26 days...	0·30 0·30

So far as these experiments go, rolling several sheets of "pure" lead foil into one compact sheet, very materially decreases the solvent action exercised upon that lead by a liquid containing nitrates.

TABLE VIII.

EFFECT OF ROLLING AND HAMMERING ON SAMPLES OF COMMERCIAL LEAD (BEAKERS).

Sample No. 1.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	Nature of Lead.	Lead dissolved, in Mgms., after	
				6 days.	14 days.
Ammonium Nitrate	0·20	25	As purchased	1·5	1·0
„ „	0·20	25	Several sheets rolled together then hammered	27·5	10·0
„ „	0·20	25	Several sheets hammered together, not rolled	trace	trace

Rolling several sheets of this lead together enormously increases the solvent action of Ammonium Nitrate upon the sample: hammering several sheets together has an opposite effect.

The following experiments illustrate the effect of rolling the original samples of commercial leads into thin sheets.

TABLE IX.

EXPERIMENTS CARRIED OUT IN CORKED FLASKS.

Sample No. 1.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	Nature of Lead.	Lead dissolved, in Mgms., after	
				6 days.	15 days.
Water	—	25	As purchased	trace	trace
Potassium Nitrate	0·20	25	„	0·3	0·4
Ammonium „	0·20	25	„	1·5	1·0
Water	—	25	Rolled out till much thinner	none	none
Potassium Nitrate	0·20	25	„	none	none
Ammonium „	0·20	25	„	trace	trace

Sample No. 2.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	Nature of Lead.	Lead dissolved, in Mgms., after	
				6 days.	15 days.
Water	—	25	As purchased	12·0	
Potassium Nitrate	0·20	25	„	5·0	
Ammonium „	0·20	25	„	12·0	
Water	—	25	Rolled out till much thinner	trace	trace
Potassium Nitrate	0·20	25	„	trace	trace
Ammonium „	0·20	25	„	—	10·0

Rolling these leads into thin sheets decreased the solvent action, except in one case, No. 2 Sample with Ammonium Nitrate, when the action was not materially influenced.

TABLE X.

EXPERIMENTS CARRIED OUT IN BEAKERS (500C.C. LIQUID IN EACH), LOOSELY COVERED WITH POROUS PAPER.

Salt.	Grams per litre.	Surface of Lead. sq. cm.	Nature of Lead.	Lead dissolved, in Mgms., after 6 days.	15 days.
Sample No. 1.					
Water	—	50	As purchased	0·8	0·8
Potassium Nitrate	0·20	50	„	0·3	0·4
Ammonium „	0·20	50	„	0·8	0·4
Water	—	50	Rolled out till much thinner	trace	trace
Potassium Nitrate	0·20	50	„	trace	trace
Ammonium „	0.20	50	„	trace	trace
Sample No. 2.					
Water	—	50	As purchased	trace	
Potassium Nitrate	0·20	50	„	trace	
Ammonium „	0·20	50	„	trace	
Water	—	50	Rolled out till much thinner	trace	
Potassium Nitrate	0·25	50	„	trace	
Ammonium „	0·25	50	„	trace	

In these cases rolling also tended to decrease solvent action. I hope to continue these experiments, and to lay the results before the Society at another time.

13. As a general rule I observed that the greater the quantity of deposit upon the lead the smaller was the quantity of lead in solution. The deposit always consisted apparently of the same salt, and appeared as light shining crystals, sometimes arranged in beautiful feathery forms upon the surface of the slips of lead. Analysis shewed that this salt was of the same composition as that noticed several years ago by Miller, viz., $2PbCO_3, Pb(OH)_2$.

14. I wish to make a correction with regard to the action of dilute solutions of Calcium Chloride upon lead. In part I. of the present papers I stated that this salt retards the

solvent action of water upon lead; this conclusion was deduced from the results of a single experiment. Since that time I have performed many experiments with solutions of the salt in question, and the general result is, that water containing small quantities of Calcium Chloride in solution dissolves decidedly greater quantities of lead than are dissolved by pure water under similar conditions.

"On the Increased Mortality from Small-Pox," by JOSEPH BAXENDELL, F.R.A.S.

At a meeting of the Chorlton Board of Guardians, held on the 19th January last, Dr. Rice, the resident medical officer at the Chorlton Workhouse, presented an interesting and valuable report on the cases of small-pox treated in the workhouse hospital during the past year; and I am induced to draw the attention of the Society to it because it corroborates in a very striking manner one of the most important of the results given in my paper "On Changes in the Rates of Mortality from Different Diseases during the twenty years 1854-73," namely, that the rate of increase in the fatality from small-pox had of late years become very much greater among adults than among infants and young children.

Dr. Rice states that 203 cases were treated in the hospital during the year, and he then gives the percentages of the vaccinated, revaccinated, and unvaccinated cases, and the percentages in each class. From these percentages I have calculated the actual numbers, and find that of the 203 cases treated, there were

149 vaccinated,
3 re-vaccinated,
51 un-vaccinated,

and amongst the vaccinated there were 17 deaths, of which 16 were adults and 1, a boy, 4 years old; and amongst the unvaccinated there were 20 deaths, of which 14 were adults and 6 under 5 years of age. Of the total deaths therefore from both classes only 19 per cent were under 5 years of age and the remaining 81 per cent were all adults. In my paper I have shown that in the 5 years 1848-52 the deaths of males from small-pox at ages under 5 years were 66·5 per cent of the total deaths at all ages, and of females 72·2 per cent, but in the 5 years 1869-73 the proportions had fallen to 31 and 36·1 per cent; and now Dr. Rice's returns give a ratio of only 19 per cent, and of course a corresponding increase in the percentage of deaths of adults, thus showing that the causes which produced the extraordinary increase in the mortality from small-pox among adults during the 20 years 1854-73 are not only still in operation but have apparently become intensified.

Dr. Rice states that of the 51 unvaccinated persons there were some who stated that vaccination had been performed but on careful examination no evidence of it could be found; it is however well known to those who have a moderate acquaintance with the literature of the subject and who have themselves made careful inquiries and observations, that the marks left by a really successful vaccination not unfrequently entirely disappear in a few years, and in numerous cases they become so indistinct that on the slightest attack of small-pox it is impossible to discern them. Jenner himself regarded with suspicion all those cases of so-called vaccination which disfigured the arms and therefore left marks which were likely to be permanent, and he contended that in such cases true vaccination had not taken place owing to the lymph used being impure or in a bad state, and in this view he was supported by some of the most eminent of his medical brethren. Now bearing this in mind, it is not at all unreasonable to assume that great numbers of cases have been recorded in the hospitals as vaccinated, in which much suffering had certainly been inflicted in the name of vaccination, but without true vaccination having taken place, while on the other hand great numbers have most probably been recorded as unvaccinated on whom true vaccination had been successfully performed. It is evident therefore that the statistics of vaccinated and unvaccinated small-pox cases are of very little value for the purposes of a scientific investigation, and that the only safe conclusions to be drawn from the entire mass of small-pox statistics we at present possess are that there has been an enormous increase in the mortality from small-pox since vaccination was made compulsory, and that this increase is still going on rapidly, but at a very much greater rate amongst adults than amongst infants and young children.

Since writing the above my attention has been drawn to the paragraphs which have appeared in some of the newspapers in reference to a proposal to introduce, in the present session of Parliament, a bill for making the revaccination of adults compulsory. I have, therefore, now no hesitation in submitting to the Society some further results which, I think it will be admitted, show more conclusively than those given in my paper that a full and impartial inquiry into the effects of vaccination is imperatively called for before further legislation on the subject takes place.

As the best test of the value of vaccination, I have discussed the small-pox statistics of London—the best vaccinated city in the kingdom—and compared the results for the five years 1849-53, before vaccination was made compulsory, with those for the five years 1869-73, when compulsory vaccination had been twenty years in operation. In the former five years, when vaccination was voluntary, and the number of vaccinated persons probably did not amount to 10 per cent of the total population, the death-rate from small-pox in London was ·292; but in the latter five years, when vaccination had been strictly carried out for twenty years, and the number of vaccinated persons was 95 per cent of the population, the rate was ·679, thus showing the extraordinary increase of 132·5 per cent.

For the reasons given above I have very little confidence in the great bulk of the returns of the numbers of vaccinated and unvaccinated cases of small-pox, but for my present purpose I will take the numbers given in the official report of the cases treated in the London hospitals during the great epidemic of 1871. Out of a total of 14,808 cases treated 2,763 died. 11,174 cases occurred in persons vaccinated, of whom 1,135 died; and 3,634 in persons unvaccinated, of whom 1,628 died. The total deaths from small-pox in London during the five years 1869-73 were 11,059, and taking the above proportion the number of deaths of vaccinated persons was 4,542, and the death rate ·293, or fully equal to the total death-rate of unvaccinated and vaccinated before a compulsory law was passed. The number of unvaccinated in 1869-73 was 6,517, and the death-rate no less than 8·009, or 27·4 times greater than the small-pox death-rate in 1849-53, and considerably above the highest rate ever experienced in this country before inoculation or vaccination were known. But unvaccinated persons like the vaccinated die from other causes besides small-pox, and it follows, therefore, if the returns of unvaccinated cases which I have used in my calculations can be relied upon, that the number of unvaccinated persons now living in London must be excessively small and utterly inadequate to account for the present epidemic. The truth, however, no doubt is, that a large proportion of the cases returned as unvaccinated were really vaccinated, and that the true death-rate of the vaccinated was greater than that given above, whilst that of the unvaccinated was much less than the calculated rate.

From the imperfect data I have been able to obtain it would appear that the proportion of deaths to cases in 1849-53 was about one-fifth. For every 100 deaths there would thus be 500 cases of sickness; but in London, after 20 years of compulsory vaccination, there was not only an enormously increased death-rate, but for every 100 deaths amongst the vaccinated persons there were 984 cases of sickness, or nearly double the number there were amongst the unvaccinated before vaccination was made compulsory; and, as I have shown in my paper above referred to, the number of deaths of persons between the ages of 25 and 65 years had greatly increased during the 20 years of enforced vaccination. So far, therefore, from vaccination having been the means of saving many thousands of lives annually, as has often been very rashly stated, the stern facts recorded in the Annual Reports of the Register-General prove beyond all question that in the best vaccinated city in the kingdom the death-rate among the vaccinated alone is now equal to that which prevailed among the unvaccinated before compulsory vaccination laws were passed; that the number of cases of illness from small-pox has nearly doubled, and that the relative number of deaths of adults has also greatly increased.

At the time of the plague it was observed that while small-pox was almost exclusively confined to children, the plague occurred principally among adults; that persons having once had an attack of small-pox were seldom known to have a second, but second and even third attacks of plague were of common occurrence; and that while the plague was found to be highly infectious, small-pox was regarded as being only contagious, or but very slightly infectious. But small-pox is now regarded as the most infectious of existing diseases, and for some years past numerous cases of second attacks have occurred among both unvaccinated and vaccinated persons, and, as I have already shown, it is now more fatal to adults than to children. It is evidently, therefore, gradually assuming the leading features of the dreaded plague; and, as it is at the same time rapidly extending its ravages in spite of vaccination, it becomes a matter of grave importance to ascertain the cause of the change in order that means may be devised to arrest its progress and counteract its effects.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 15th, 1877.

E. W. Binney, Esq., F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On the Occurrence of Cratægus oxyacanthoides (Thuill.) in the neighbourhood of Manchester," by Charles Bailey, Esq.

In a paper already published in the "Proceedings" of the Society, "On the Hawthorns of the Manchester Flora," I pointed out that three species of *Cratægus* occurred in this neighbourhood. The most frequent of these is the *Cratægus monogyna,* Jacq., and it is easily recognised by the diverging veins of the leaf and by the single style. A second species, the *C. kyrtostyla,* Fingh., occurs in Botany Bay Wood, near Worsley; but it has not, to my knowledge, been detected in any other locality in the neighbourhood, possibly on account of its being passed over for *C. monogyna*; its leaves are similar, but its one to two-styled flowers, and pubescent calyx and peduncle will always distinguish it from that species. The third species, *C. oxyacanthoides*, Thuill., has hitherto been represented in our flora by a single plant, at Marple; but the specimens now exhibited from three new stations, discovered last year near Manchester, extend the area of its distribution in this part of England; these stations are Cressbrook Dale and Wardlow Hay Cop, in Derbyshire, and Taxal, in Cheshire. The specimens of *Cratægus oxyacanthoides*, from Derbyshire, clearly belong to the typical species described in English Botany, ed. iii., and are at once identified by the converging veins of the leaf and by the two-styled flower. Of the three new stations now reported for this species, the one situate on the eastern flank of Wardlow Hay Cop gives the best evidence of the species being indigenous with us, although only two or three bushes were to be found. The Cressbrook Dale station, which is on the western side of Wardlow Hay Cop, furnishes one bush in a hedge by the road side, and may have been introduced from some distant locality. The nativity of this species in the Taxal station is much more doubtful, as there is but one old shrub growing on the remains of a deserted lawn; this plant, further, shows an approximation to *C. monogyna* in the variation of its leaves, and to *C. oxyacanthoides* by its two styles; in many respects it approaches an Austrian species, the *C. intermedia*, Schur., but I am unable to refer it to that species from the absence of authentic data. I exhibit a set of British Hawthorns in illustration of these remarks, and amongst the varieties is a fruiting example of *C. monogyna* var. *pinnatifida*, from Arnside, Westmoreland.

"List of Desert Plants collected at Ramleh, near Alexandria, Egypt, from September, 1875, to April, 1876," by H. A. HURST, Esq.

In placing before the section a list of the plants collected during the winter of 1875—1876, at Ramleh, Egypt, I may remark that there is another place of the same name in Palestine, between Joppa and Jerusalem. In both cases I believe the name to be derived from the Arabic R-M-L, signifying sand; its derivative Ramleh being used to express a desert. It is a narrow slip of undulating sandy ground, between the Lake Maadieh on the right hand, looking north, and the sea on the left. It abuts on the ground, where Cæsar had his camp, B.C. 48, remnants of which remained till lately, when its traces were almost completely effaced by Ismael Pacha, the stones being taken to build one of his many palaces. This is a very striking building, at present uninhabited, and one of Egypt's many monuments to waste. Ramleh is interesting to the Englishman, as the site of the Battle of Alexandria in 1801, when the English, under Sir Ralph Abercromby, defeated the French, who were then in possession of Egypt. It will be remembered that the leader fell, mortally wounded, in the action.

At the present moment the place bears much the same relation to Alexandria which Bowdon does to Manchester, or Richmond to London. There is a railway running to it from Alexandria, the farthest station being about five miles distant from Cleopatra's Needle and the ground is sprinkled over with well built villa residences, to which English, Greek, and Italian, and other residents of Alexandria resort during the warmer months of the year some even making it their permanent abode. The dwelling houses cease entirely about six miles from Alexandria, where uninhabited desert is reached. Botanically speaking, the district may be divided into three portions, the sea coast, the desert, and the irrigated margins of Lake Maadieh, the plants of which the following is a list were collected in the two former districts. I hope on a future occasion to bring before you those found on the irrigated land in the vicinity of Ramleh and Alexandria generally.

It is stated that the migratory birds pass from Europe to Africa and Asia by three routes, the most westerly being by Gibraltar, the central by Malta, and the most easterly by Greece, Cyprus, and Egypt. I am inclined to think that the point for which these last birds direct their course is Ramleh; the coast being comparatively conspicuous both from its height and from the waves generally breaking on its shores. This may possibly account for some ot the plants there found.

According to Boissier, one of the most ancient botanical explorations was that of Prosper Alpius, in the sixteenth century; Lippi also travelled in the country at the commencement of the eighteenth century; but, although it may be true that

"Vixere fortes ante Agamemnona,"

still it is from Linnæus and his followers that we in the present age date our knowledge.

Forskahl (a pupil of Linnæus) arrived in 1761 in Egypt, and it was from his Flora Egyptiaco-Arabica that Europe first became acquainted with the Egyptian flora. He appears to have been a most extraordinary man. He was robbed and ill-treated and (it is reported) sold as a slave in Egypt. Though suffering from constitutional timidity, and often nearly broken down by sickness, the extraordinary energy and perseverance with which he examined the vegetation of Egypt and Arabia produced a work unsurpassed in its day. The hardships he underwent prevented his surviving to publish it. We are indebted to his friend Niebuhr for its appearance, and it is remarkable as being one of the first books in which the relation of vegetation to climate is taken as an object of importance. For many years the Flora Egyptiaco-Arabica was the only good account we had of the plants of those countries. In the following list I have aimed at giving his synonymes of all the plants enumerated.

Delile subsequently published in the Botanique de l'Expedition d'Egypt a very valuable account of its vegetation, with excellent engravings.

In later years Ehrenberg and Hemprich, Dr. Ruppel, Aucher Eloy, Boné, Schimper, Boissier, Kralik, and more recently Samaritani, Cadet de Fontenay, and Dr. Gaillardot, have examined this district.

It does not appear to have attracted the attention of any Englishman except Henry Calvert, Vice-Consul at Alexandria, to whom I am greatly indebted for assistance.

Judge Latourneax, of the new Courts in Alexandria, an able botanist from Algeria, has this year I believe taken to Paris a very complete list of the Ramleh plants.

Meanwhile Dr. Oscar Schneider has published an interesting though incomplete list in the "Isis,"Dresden Heft 111, 1871. The authority on the subject to-day is however Boissier's Flora Orientalis, the first volume, printed in 1867, hardly does justice to the Egyptian Flora, but the subsequent volumes leave nothing to be desired and are above my feeble praise. It is a magnificent work, worthy of any age or country, and will rank with Decandolle's Prodromus, though only local in application instead of cosmopolitan as that work is.

[The List of Plants will probably appear in the Society's Memoirs.]

Ordinary Meeting, March 6th, 1877.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Mr. Thomas Mackereth and Mr. Alfred Brothers were appointed Auditors of the Treasurer's Accounts.

The **President** said that since his Notice of Some Organic Remains from the Schists of the Isle of Man, read before the Society on the 26th day of December last, he had again examined the slates at Derby Castle, on the north side of Douglas Bay. These beds of hard slates are perforated with holes, generally of an oval form, about a foot above high-water mark and the same distance below. In them he found live specimens of the *Littorina littorea,* and from the general resemblance of the shape of the holes to the mouth of that shell it appeared to him that it was probable they might be made by that animal. He certainly had never heard of its boring or excavating powers; but he brought it before the Society for the purpose of obtaining information on the subject. It was well known that the teeth of the whelk had great cutting powers, but he had not seen it noticed that the *Littorina* had.

Professor **Balfour Stewart**, LL.D., F.R.S., gave an account of the Sun Spot and Magnetical Observations which have been made at the Kew Observatory, and exhibited diagrams showing that the maximum and minimum ranges of variation of magnetic declination generally lagged behind those of solar spot frequency by a period of about six months.

"Note on Early Anticipations of a Magnetic Telegraph," by Professor W. S. JEVONS, M.A., F.R.S.

Mr. Harry Grimshaw, F.C.S., drew attention, on February 20th, to the very interesting fact that physicists had two or three centuries ago imagined the possibility of making distant communications by the use of the magnetic needle, and he has pointed out a work containing allusions to a magnetic telegraph. This curious subject has not received the attention which it seems to deserve, but it was not wholly unknown. The Abbe Moigno, in his Traité de Télégraphie Electrique (Paris, 1852), alludes to what he calls this "Charmant rêve, ou operation necromancienne," and he points out that Addison had quoted the remarkable verses of Famianus Strada in the *Spectator*, No. 241. Addison speaks of "a chimerical correspondence between two friends by the help of a loadstone." Strada's remarkable lines are also quoted and translated in Mr. George Dodd's account of "Railways, Steamers, and Telegraphs: a Glance at their recent Progress and Present State" (Chambers, 1867).

About ten years ago I spent some trouble in investigating this curious anticipation of the telegraph, but only published the results in the form of a brief anonymous article in a weekly newspaper. I found allusions to a magnetic telegraph running through many scientific, or quasi-scientific, works of the 16th and 17th centuries. Sir Thomas Browne, in his *Pseudodoxica Epidemica,* says:—"The conceit is excellent, and, if the effect would follow, somewhat divine;" and he speaks of it as a conceit "whispered thorow the world with some attention, credulous and vulgar auditors readily believing it, and more judicious and distinctive heads not altogether rejecting it." Sir Thomas, it would seem, submitted the matter to experiment, but found that though the needles were separated but half a span, when one was moved "the other would stand like Hercules' pillars."

Joseph Glanvill, in his *Scepsis Scientifica* (1665), discusses the objections of Sir Thomas Browne, and concludes that "there are some hints in natural operation that give us probability that it is feasible." How can we read without wonder these words written by Glanvill more than 200 years ago. "Though this pretty contrivance possibly may not yet answer the expectation of inquisitive experiment, yet 'tis no despicable item that by some other such way of magnetick efficiency it may hereafter with success be attempted, when magical history shall be enlarged by riper inspections; and 'tis not unlikely but that present discoveries might be improved to the performance." It is evident that Glanvill treats the matter quite seriously as a scientific possibility. The Marquis of Worcester probably refers to the magnetic telegraph when he speaks of "intelligence at a distance communicative, and not limited to distance, nor by it the time prolonged." (Dirck's Life of Worcester, p. 357.)

I tried to trace these notions to the first inventor, but, as might be expected, without much success. Strada attributed the invention to the celebrated Cardinal Bembo, the secretary of Leo X., but as Bembo (who died 1547) was a historian and literary character, it is hardly likely that he would originate a scientific conception of the sort. The earliest books in which I have found allusions to a magnetic telegraph is the *Natural Magic* of Baptista Porta, published in 1589. In the seventh book he describes the "wonders of the magnet," saying in the preface, "I do not fear that with a long absent friend, even though he be confined by prison walls, we can communicate what we wish by means of two compass needles circumscribed with an alphabet." In the eighteenth chapter of the same book, he describes the experiment of putting a magnet under a table and moving thereby a needle above the table. This experiment, as Porta remarks, was known to St. Augustine, and an exact description will be found in his "De Civitate Dei," a work believed to have been begun A.D. 413. It seems probable that this passage in St. Augustine suggested the notion either to Porta, Bembo, or some early Italian writer, and that thus it came to be, as Sir Thomas Browne says, "whispered thorow the world."

"Note on a Passage in Strada containing a Prevision of the Electric Telegraph," by WILLIAM E. A. AXON, M.R.S.L., F.S.S.

The interesting quotation from Hakewill's Apology brought by Mr. Grimshaw before the last meeting of this Society, seems to need an additional word of comment. Hakewill, it will be remembered, quotes a passage of Latin verse from Strada in which he supposes the loadstone to have such virtue that "if two needles be touched with it, and then balanced on separate pivots, and the one be turned in a particular direction, the other will sympathetically move parallel to it. He then directs each of these needles to be poised and mounted on a dial having the letters of the alphabet arranged round it. Accordingly, if one person has one of the dials and another the other, by a little prearrangement as to details, a correspondence can be maintained between them at any distance by simply pointing the needles to the letters of the required words." The date of the first edition of Hakewill's Apologie or Declaration of the Power and Providence of God in the Government of the World is 1627; but the work of Strada's from which he quotes was published ten years earlier. Famianus Strada was born at Rome in 1572, and his Prolusiones Academeciæ et Paradigmata Eloquentiæ appeared at Rome in 1617. Several editions of his Prolusiones have been printed in this country. The particular poem referring to the loadstone has been translated into English and is printed in "The Student or Oxford and Cambridge Miscellany," 1750. The passage is referred to by Addison in a paper in the

Spectator, No. 241, and in the *Guardian*, No. 119. In the former of these he adds: "In the meanwhile, if ever this invention should be revived or put in practice, I would propose that upon the lover's dial plate there should be written not only the four-and-twenty letters, but several entire words which have always a place in passionate epistles: as flames; darts; die; language; absence; Cupid; heart; eyes; hang; drown; and the like. This would very much abridge the Lover's pains in this way of writing a letter, as it would enable him to express the most useful and significant words with a single touch of the needle."

Akenside, in speaking of the laws of association and sympathy, says:—

For when the different images of things,
By chance combined, have struck the attentive soul
With deeper impulse, or connected long,
Have drawn her frequent eye; howe'er distinct
Th' external scenes, yet oft the ideas gain
From that conjunction an eternal tie
And sympathy unbroken. Let the mind
Recall one partner of the various league,
Immediate, lo! the firm confederates rise,
And each his former station straight resumes:
One movement governs the consenting throng,
And all at once with rosy pleasure shine,
Or all are saddened with the glooms of care.
'Twas thus, if ancient fame the truth unfold,
Two faithful needles, from the informing touch
Of the same parent-stone, together drew
Its mystic virtue, and at first conspir'd,
With fatal impulse quivering to the pole;
Then, though disjoin'd by kingdoms, though the main
Roll'd its broad surge betwixt, and different stars
Beheld their wakeful motions, yet preserv'd
The former friendship, and remembered still
The alliance of their birth; whate'er the line

Which one possessed, nor pause nor quiet knew
The sure associate, ere with trembling speed
He found its path and fixed unerring there.
(*Pleasures of the Imagination,* Book iii.)

This passage is cited in Dr. Thomas Brown's Philosophy of the Human Mind, (16th ed., 1846, ii. 220).

Strada's fancy is also referred to in the Abridgements of Specifications relating to Electricity and Magnetism (1859, p. xliii), "Saturday Review," August 21, 1858, p. 190; Moigno: Traité de Télégraphie Electrique, p. 58–59, and in Keddie's Literary and Scientific Anecdotes, where several of the above references are to be found.

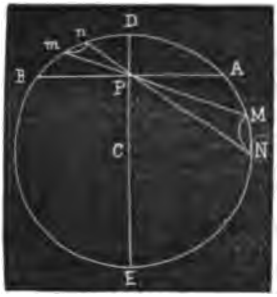
It is to be noticed that the effect is ascribed to the power of sympathy. This was an important doctrine of the time, and reached transcendental heights in the hands of Sir Kenelm Digby.

"On the Law of Force when a Thin Homogeneous Spherical Shell exerts no Attraction on a Particle within it," by J. H. POYNTING, B.A., B.Sc.

If a homogeneous thin spherical shell of uniform thickness exert no attraction on a particle within it, then the law of the force is the law of nature.

Professor Maxwell uses this proposition (Electricity, vol. i., § 74) to deduce the law of the force between electrified bodies, and shows that it proves far more conclusively than any direct measurements of electrical forces that the law is that of the inverse square. It would therefore be an advantage to have a simpler proof of such an important proposition than that given by Laplace (Mec. Celeste, liv. ii., cap. 2) and followed by Maxwell. The following seems more simple as it requires neither integration nor the solution of a functional equation:—

Let P be any point inside the spherical shell, C the centre of the sphere, DPCE the diameter through P, and APB



perpendicular to CD. In Newton's proof of the proposition that if the law of attraction be that of the inverse square the force at P is zero, the surface is divided into an indefinitely great number of opposite elements by small cones having their vertices at P, and the attractions of each of these pairs of elements are shown to balance each other. We shall first shew that if the attraction at P be zero, then it follows inversely that for at least one position, if not for all positions of the cone MP*m*, besides the position APB, the attractions of the opposite elements balance each other, and we shall thence prove that the law of attraction must be that of the inverse square.

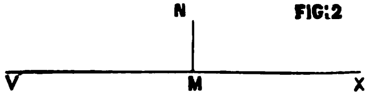
Let us suppose the cone with vertex at P to move round from the position where AB is its axis to any other position MP*m*. At AB the attractions of the opposite sections on P are equal whatever the law of the force. As the cone leaves AB let us suppose the resultant attraction of the two opposite elements to be no longer zero, but to act, say, towards the centre side of APB. Then it will either continue towards that side as the cone moves all the way round from APB to BPA or it will vanish at some position, and then act in the opposite direction. In the first case we should have a number of forces all acting from P towards the same side of APB, whose resultant is zero. Then each separate force must be zero. In the second case the resultant attraction of the opposite sections vanishes somewhere between AP and EP. Then for at least one position of the cone, besides the position APB, the resultant attraction of the opposite sections vanishes.

Since this is true for any position of P, we can show that the law of the force must be that of the inverse square.

In the position where the two opposite sections exert equal attractions, two sections of the same thickness perpendicular to the axis of the cone would also exert equal attractions, for they would bear to each other the same ratio as the two oblique sections made by the sphere since these two oblique sections make equal angles with the axis of the cone. Then what we have proved is that for every position of P there are two different distances for which the attractions of the sections of a small cone of equal thickness on a point at its vertex are equal.

Let us take VMx to represent the axis of a cone of very small angle of which V is the vertex.

FIG:2



At any point M draw an ordinate MN to represent the attraction of a section of the cone at M of small given thickness on a point at the vertex. Then N will trace out a curve as M moves along Vx.

Now take a spherical shell of thickness equal to the thickness of the sections of the cone, and of radius nearly equal to VM, where M is any arbitrary point in Vx. Take a point near the centre of this sphere. As a cone moves round with this as vertex, its sections by the sphere must be always at distances very nearly equal to the radius from the vertex, and by what we have proved above for some position of the cone the attractions of the opposite sections must be equal. Therefore (in fig. 2), for two distances very nearly equal to VM the ordinates must be equal to one another. Then the tangent to the curve near N must be parallel to Vx. But M is arbitrary, for we can take the sphere of any size, therefore at all points the tangent to the curve is parallel to Vx, and therefore the curve must be a straight line parallel to Vx, or the attractions by sections of the cone of equal thickness are constant wherever the sections be taken. But the sections are proportional to the direct square of the distance, and therefore the law of the attraction must be that of the inverse square of the distance.

Mr. **Arthur Wm. Waters**, F.G.S., read a paper entitled "Enquiries concerning a Change of Position of the Earth's Axis," in which he said that the cause of the greater warmth in high latitudes during the tertiary period has not, according to the opinion of many, yet received any satisfactory solution.

The arctic Miocene flora was considered. 356 Miocene species have already been determined from latitude 70–77 N. in Spitzbergen and Greenland, and include Taxodium (swamp cypress of Texas), Sequoia ,birch, lime, oak, beech, plane, and even magnolia; so that Prof. Heer, by comparison of the localities of these, says that the temperature must have been 30° F. warmer than at present. Fossil floras of the Cretaceous, Jurassic, and Carboniferous periods have been discovered within the arctic circle. Most of the plants are unable to resist a severe cold, besides requiring a warm summer, and it seems difficult to accept the fact of their flowering and ripening their seeds, where the winters are so long and the summers so short, and, apart from the lower temperature, where the amount of light is so much diminished.

Several theories have been brought forward to explain the cold of the glacier period, the generally received one being that of Mr. Croll, that the glacial periods were brought about *indirectly* from an increase in the eccentricity of the earth's orbit, modified by the obliquity of the eliptic. In the longer and colder winters more snow fell, which the summer could not melt away, so that the earth now covered gets little of the warmth of the sun. As this explanation has not always been thought quite satisfactory with regard to the greater warmth, the change of the position of the earth's axis has from time to time been suggested on various grounds.

It was suggested that there are three causes which might change the position of the axis, viz., distortion of the earth by continental and local upheaval altering the centre of gravity, and thus changing the position of the axis; the removal of water by elevation of land displacing the ocean; and the removal of matter in solution; and these two last, though not considered to be the most important in amount, were considered more in detail.

According to Mr. T. Millard Reade, in a paper on Geological Time, read as a presidential address to the Liverpool Geol. Soc., 1876-77, about one ton of solid matter is removed in solution by the drainage in each square mile, so that it was shown about 5000 million tons are removed from the land surface each year; so that in ten years a weight equal to that of Vesuvius is removed from the land to the oceanic area by this means; and as there is more land in the northern hemisphere, this gives a gain for the southern hemisphere of 3230 million tons over the northern. If the earth is divided into a land and water hemisphere with England as a centre, the gain of the weight of the water hemisphere is about 4300 million tons, or one Vesuvius in twelve years, the place of greatest gain being about 45° S. and the greatest loss 45° N. in antipodal positions.

A statement of Sir G. B. Airy's in the "Athenæum," 1860, was taken to see what effect special alteration would have.

Removing a weight equal to that of Asia 1000 feet high from the centre of the land hemisphere, and adding a similar weight at the antipodes in a sinking Pacific Ocean, leaving the remaining portion in each hemisphere balanced by the natural configuration, would give an alteration of from 18—27 miles. This alone would require 13 million years at the present rate of denudation, but there are many causes, some of which were mentioned, which would very much reduce the time required for this amount of "soluble denudation," so that it might be reduced to one or two million years, and the vast thickness of calcareous rocks, which are only the record of others from which they were partly formed, shows how many times such areas must have been transported from land to sea.

The sinking of an area equal to the continent of Asia to the mean depth of the ocean would bring a weight of water sufficient, if the antipodes were a suboceanic rising area, to displace the position of the axis 40—60 miles by the same manner of calculation. It may be thus seen that these may be disturbing or starting forces, but do not give a large amount of change directly, and that the one to three degrees which Mr. George Darwin, M.A., allows is all that we should expect in recent geological times unless there is some cumulative effect.

Mr. Waters maintained that if the change was caused by addition of weight, then the earth in readjustment would cause phenomena equivalent to an elevation in those semi-hemispheres from which the maximum bulge has been removed, displacing, if it should be an oceanic area, an amount of water to be placed in another region; the maximum effect of each degree of change is $\frac{1}{226}$ of the weight of the bulge, and the possibility of a redistribution of land and sea preventing a change in an opposite direction of the motion of the poles was pointed out.

The astronomical objections are that any such movement of the axis would be discoverable from the earth's and moon's motions, that is, by precession and mutation of the equinoxes, which are caused by the attraction of the sun and moon on the equatorial bulge. It is from no sufficient change in these motions that we have been told the figure of rotation has not altered in 3000 years (the limit of known observation), but this has been based upon a preternatural rigidity of the earth which is not now maintained by all physicists.*

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* Sir William Thomson says in his Glasgow address: "A slow distortion of the earth as a *whole* would never produce any great angular separation between the instantaneous axis and axis of maximum moment of inertia for the time being."

It has been seen how the forces under consideration may have acted in opposite directions, and a consideration of recent geological phenomena shows that while large areas have been elevated it has not taken place steadily and uninterruptedly, but that there have been elevations and subsidences (or kindred phenomena) many times repeated, so that if we turn to the north of Europe, in Belgium, or to Italy, we find for a general elevation of a few hundred feet to have taken place since the middle of the tertiaries there have been subsidences and elevations of many thousand feet in each direction. Now, with the number of forces at work and the irregular distribution of land and sea, it may be said that a reversal of conditions in one part could not take the axis back to exactly the same place; in other words, the axis might tack.

The great changes in the tertiaries were briefly considered, showing how much change of level has taken place.

Mr. Waters said the points he wished to bring forward are, that a change of the position of the axis would elucidate many facts which have not yielded to any other explanation, and that a change of the position must take place, but that only a small amount could be directly proved; but if there is a cumulative effect, then it may be explained, and the idea thrown out for examination by physicists was whether the frequent changes in direction which are caused by forces working in various parts of the world would not thus give a sufficient increase to the amount calculated.

When speaking of recent papers bearing on this subject the author read the following foot-note:—

Since this paper was written, "Le Déplacement Polaire" of Dr. Jules Carret has come into my hands. This little work is written to demonstrate from various grounds that the position of the axis has changed, but without enquiring into the cause. The greater part of the book is devoted to proving that the present distribution of land and sea can only be explained by such a change, and this he thinks explains the polar land area and the antipodal position of nearly all the land to water areas. And he shows that the effect of a change of position of the axis with the unequal diameters of the earth, which are unequally divided by the centre of gravity, will be to cause the land to be antipodal to the water, and the slight exceptions are near to the circumference, dividing the globe into land and water hemispheres, which is where the exceptions would be expected.

By these exceptions he concludes that the poles have moved in a curved direction. If we divide the earth by a plane (grand circle polaire) perpendicular to the equator and to the direction in which the position of the poles have been changing, the points of intersection at the equator form two pivots for this motion, and here the effect of readjustment will be a minimum, while before and behind will be an area of elevation and submergence respectively. If the motion is curved, the plane (*g.c.p.*) we have just supposed must cut the tangent of this curve at right angles. The changing position of this tangent changes the position of the plane, and the points of intersection are removed from the elevation to the submergence area on one side, and the opposite points from the submergence to the elevation area, so that land antipodal to land is the consequence.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 12th, 1877.

W. Boyd Dawkins, F.R.S., President of the Section, in the chair.

Contributions to the Life History of *Acidalia Contiguaria* (Hübner), communicated by Joseph Sidebotham, Esq., F.R.A.S.

This species of geometer is one of the rarest in Great Britain, and the following notes upon it may be of interest to the members of our Society.

Acidalia Contiguaria was, I believe, first captured by the late Mr. Weaver, on the road between Conway and Penmaenmaur, about the year 1840. I have in my possession one of the original specimens; it has since then been met with occasionally, always in the same district, a little East or West of Conway. The larva is supposed to seed on *Empetrum nigrum*. Mr. Greening, of Warrington, has, I believe, reared the moth from larvæ, but I have seen no specimen nor any record, although several friends of mine have specimens in their cabinets, supplied by him.

During my stay at Llandudno last summer on account of health, my friend Mr. Capper, of Liverpool, went over many times to the hills about Conway, and succeeded in finding a few specimens of *Acidalia Contiguaria*, some of which he brought to me in a living state; one of the moths laid a few eggs on the 16th of July. The larvæ appeared on the 22nd; they were very small, so as to be scarcely visible without a magnifying glass. Not being able to procure *Empetrum nigrum*, I put into a small bottle of water in the box or cage sprigs of *Erica cinerea* and *Polygonum aviculare*, both of which they appeared to eat; as the latter was more easily procurable, they were fed chiefly upon it, although I supplied them with *Erica* when practicable. The larvæ varied considerably in colour, from an ochre brown to grey. On the 22nd of August, just one month from their being hatched, some of them began to spin their cocoons; these were formed of very loose silk mixed with small grains of peat, with which I had furnished them, and were fastened in the angles of the small cage. The first moth emerged from pupa on the 9th of September, and of the 8 or 9 that had spun up, most of them had become moths by the end of the month. The remainder of the larvæ are still in the larva state, and will probably not spin up before Spring. I was anxious, if possible, to perpetuate the breed (especially as the species may be extirpated or become very rare, owing to the heather and grass on the hills where it occurs having been all burned shortly after the middle of July).

The first female escaped, but from another I obtained about 80 eggs; these hatched on the 8th October. The cold weather had almost destroyed *Polygonum aviculare*, so it became important to find a plant the larvæ would feed upon which was procurable during the Winter, so I placed in a bottle sprigs of *Stellario media*, *Senecio vulgaris*, and *Erica Mediterranea*, along with such *Polygonum aviculare* as I could meet with, and I was pleased to find that the young larvæ took very kindly to both the former species. Shortly afterwards I had to leave home for the South of France, and left the larvæ in charge of my son. The report from him, dated Nov. 20th, says, the larvæ are feeding and thriving well: whether they will spin up before Spring or not I cannot say, but I think it probable they will become partially torpid during the Winter months, and feed up and spin in the Spring. The larvæ hatched in July which did not spin up are still in the larva state, and take very little food, and I think they will continue in the same condition until towards next May.

Additions to the List of Shells from Cymmeran Bay, Anglesea. Part IV. By John Plant, F.G.S.

During the spring and autumn of 1876 the shores were assiduously searched, and the sea dredged along the coast, chiefly on the shingly banks where the oysters and Cardium echinatum are found so abundantly. The bottom of the oyster boats were carefully examined and afforded many choice specimens of the smaller shells with the animal either living or dead within the shells.

The season, however, was not at all favourable for shore-strewn shells compared with the two seasons preceding. Stormy weather was not so frequent or so powerful, and there was an absence of the great rollers which effectually plough up the seaweed and shingle from the banks lying about two miles off the shore. As a rule, great storms bring the fruitful harvest for the naturalist. Searching these sandy shores after ordinary or quiet tides is generally an unprofitable labour.

Thirty-three species have been added to the former lists from this locality, during this season. Amongst the *foreign* things which came up in the dredge were valves of *Perna ephippium* and a young *chama,* relics from a wreck which took place in the bay a few years back to a vessel laden with pearl shells from the Indies. At present one part of the rocky bay is strewn with the great valves of the large Pernæ, the rocky bottom preventing them from being dredged.

ADDITIONAL SPECIES.

155. Pholas dactylus; rare.
156. Saxicava arctica; several dredged and living shells agree so well with the figures and descriptions of authorities, that I do not hesitate to identify my specimens with the species. Forbes gives Anglesea as a locality for it.
157. Mya arenaria; found on the shore at Aberffraw.
158. Solecurtus candidus; found also at Aberffraw.
159. Tapes virginea; dredged.
160. Venus casina; dredged alive. In the "Corrections" which I communicated to the Section, March, 1876 (Proc. vol. xv. p. 139), this species was eliminated under the impression that my former specimens were worn *Venus verrucosa.* However that may be, these present living specimens prove that V. casina is native in the bay.
161. Astarte sulcata; dredged living of good size. A very young one had previously been found on the shore.
162. Astarte triangularis; three valves of this well-marked species were found in an oyster boat.

163. Cardium nodosum; a few perfect shells from the oyster boats, showing that it lives in the bay in about 15 fathoms water.
164. Kellia suborbiculata; this occurs as a small variety only half the normal size.
165. Crenella discors; dredged.
166. Crenella nigra; one valve agreeing accurately with true specimens of this species in the Museum collection by Mr. Charlesworth.
167. Leda caudata; three valves of this pretty species found in the oyster boats.
168. Anomia aculeata (?) I am not certain about this; the shells are small, and it may possibly turn out to be the fry of a patelliformis.
169. Trochus montagni; one specimen dredged.
170. Trochus tumidus; a good number of this shell was dredged.
171. Rissoa costulata; one specimen.
172. „ proxima; eighteen specimens.
173. „ soluta; nineteen specimens.
174. Rissoa rubra; five perfect specimens. Of the Rissoæ named in former lists the following were found in considerable numbers:— R. striata, ulvæ, parvus, cingillus, costata, and labiosa; a variety of R. parva was found ornamented with coloured zigzag lines like a Phasianella pullus.
175. Skenea divisa; two specimens. S. rota has only once been found, whilst S. planorbis is abundant.
176. Cerithium reticulatum; found abundantly.
177. Aclis nitidissima; rare.
178. Chemnitzia formosa; rare.
179. Otina otis; two specimens of this delicate shell.
180. Velutina lævigata; ten specimens.
181. Lamelaria perspicua; sixteen specimens.
182. Cerithiopsis tubercularis; rare.
183. Trophon muricatus; three specimens dredged.
184. Cylichna truncata; four of this well-marked shell.
185. Cylichna mammilata; found in good numbers.
186 Philine catena; fourteen specimens.
187. Convolvulus bidentatus; sixteen specimens.

To the present time the number of species collected at Cymmeran amounts to one hundred and eighty-seven; it is quite possible that further researches, especially by dredging, will increase the list to two hundred species. Of the species dredged alive, which have been only previously found on the shore, I may particularise the following:—Saxicava rugosa, Venus ovata and fasciata, Lucina borealis—to the specimens of this last species I may remark that although I am bound to agree with the opinion of Mr. R. D. Darbishire that none of them belong to Lucina leucoma, I must maintain that it would be taken by any less authority for the latter species. It appears to be a permanent and small variety of L. borealis; for out of more than 200 specimens taken at Cymmeran, the largest valve measures a little over half an inch long and barely half an inch wide. Most of the specimens are less, and yet are adult. A normal L. borealis from other localities is an inch an a half long and of corresponding height. Other species dredged are, Crenella marmorata and discors, Nucula, Pectunculus, Pecten maximus, opercularis, pusio, varius, Danicus and tigrinus, Anomia several species, Pileopsis, Trochus ziziphinus, Nassa reticulata, Trophon clathratus and muricatus.

Cypræa Europæa is an abundant species, and occurs so fresh and bright that there is no doubt it lives in the bay, especially as the young shells are often found on the sands.

It is singular that a shell so abundant in most localities as Turritella communis should be so rare at Cymmeran; even Aporrhais is much more plentiful than Turritella.

The following twenty-five species are the commonest, following in the order given:—

Patella vulgata, athletica and pellucida, Donax anatinus, Littorina 3 species, Pecten varius, pusio, opercularis and maximus, Mactra 2 species, Venus striata, Tellina solidula, Tapes pullastra, Cardium echinatum and edule, Modiola modiolus, Anomia ephippium, Ostrea edulis, Trochus umbilicatus, Cypræa, Buccinum undatum, Cyprina Islandica.

Footprint of "Dinornis," Bones of "Dinornis" and "Dodo." By JOHN PLANT, F.G.S.

The author stated that in 1839 a fragment of a femur of a bird was sent from New Zealand to Professor Owen. From it the learned osteologist felt authorized in establishing the genus "Dinornis," to include a race of giant Struthious birds which had not been seen alive in New Zealand since Pleistocene times. In the course of the thirty-seven years since 1839 the remains of these birds have been found in immense quantities and in many places in New Zealand, buried either in peat, mud, and sand on the level plains, marshy tracks, or shores, but recently raised above the tides. Professor Owen has given anatomical details of about fourteen species, ranging from 17 feet high down to the size of the South American Rhea. His descriptions are to be seen in twenty "Memoirs on the Dinornis," published in the "Transactions of the London Zoological Society."

Bones of the Dinornis are found associated with the debris of cooking-pits and kitchen-middens of the aborigines of New Zealand. The bones show evidences of having been burnt or roasted and with cuts upon them as with flint knives. There is a tradition that the old natives fed upon the flesh and made cloaks from the gaudy coloured feathers of the Moa. Egg shells in fragments have been found in these cooking-pits, and recently, in 1876, footprints of the Dinornis have been discovered by Mr. C. J. Bold, C.E., on the sandy shores at Poverty Bay, on the western coast, North Island, at the spot where Captain Cook first landed in New Zealand in 1769 and had his hostile encounter with the natives. The great navigator describes Poverty Bay as an "unfortunate and inhospitable place, named by the natives Ta-oni-roa, or the Long Sand. The entrance is commanded by high and steep white cliffs, whilst the low shores of the Horse-shoe Bay are covered with white sand, beyond which the country is diversified with wooded hills and valleys."

The foot print from this place is upon a light and friable sandstone, it measures fully 9 inches from the heel to the tip of the claw of the middle toe and is 8 inches in width at the broadest part of the tread, the depth of the impression at the heel is quite one inch, and also the end joint of the middle toe is deeply impressed; it gives one the notion of a bird walking over firm but wet sand, after the sand is treated with nitric acid, and repeated washing with filtered water, so as to get rid of the fine white sediment, the residue, about five sixths, shows under the microscope a curious sand made up entirely of jagged splinters of transparent and opaque quartz and felspar, with fine grains of black magnetic iron and schorl meagerly scattered in the mass. There is not a trace of lime or of alumina, the sandstone is crumbly and so friable that it is a wonder how the footprint was ever preserved. No doubt it was preserved by being covered with blown sand soon after it was made, and time has set the quartz splinters in a finely divided powder of felspar.

The author exhibited the bones of a leg and foot of "*Dinornis Casurinus*," from peat beds on the Canterbury Plains, and a number of bones of the *Dodo*, exhumed from fine black mud by his brother Mr. Francis Plant in 1869, near Port St. Louis, Mauritius.

"Upon Specimens from the Fresh Water Deposits of La Limagne d'Auvergne in Central France," by Mr. MARK STIRRUP, F.G.S.

The author referred to the district as one of great interest both to geologists and palæontologists, and one made famous by the labours of those veterans, the late Poulett Scrope and Sir Charles Lyell. These freshwater deposits are of the tertiary age, and occupy a valley plain of considerable extent between the two parallel granitic ranges of the Monts Dômes and the Farèz.

The order of deposition and the more interesting varieties of the limestone with their included organic remains were commented on, also the deposits of gypsum and silex in various forms. Specimens were exhibited of the indusial limestone, which is entirely built up of the larval cases of a species of Phryganea or caddis fly, solidified by the deposition of lime or travertin. Another variety is composed of the valves of that minute crustacean the fresh water Cypris, similarly preserved by an incrustation of lime. With these were also shown specimens of the shell limestone containing Helix beautifully preserved. Other land and freshwater shells, as Lymneus, Planorbis, &c., also occur.

One variety of limestone in a crystalline condition containing Potamides is quarried as a marble for building purposes.

These limestones are all of chemical origin and due to mineral springs, which are still numerous over the whole district, but were doubtless more abundant in the past. La Limagne is only one of three or four basin-like depressions which occur in this portion of France. Though now totally separated by lofty granite plateaux or volcanic mountains, it is very probable that these freshwater deposits covered the whole intervening area. Monsieur Julien, Professor at the Faculty of Sciences at Clermont, sets forth the result of his personal researches as teaching that these pretended tertiary basins are but fragments of a continuous lacustral formation which covered the whole or part of the central plateau and united the great tertiary basins of Cantal, Velay, and the Limagne. He demonstrates that systems of enormous faults have dislocated these basins, and particularly the ground of La Limagne. Differences of level of upwards of 1000 feet occur in these fresh water deposits which demonstrate that great oscillations and disturbances took place since their deposition. These movements may with safety be assigned to that period of volcanic activity represented by the extinct craters of Auvergne.

Errata.

Vol. XVI. No. 9.

Page 157, For "Cræetagus," line 4 from bottom, read "Crataegus."

Page 158. For "variation," line 5 from bottom, read "venation."

Ordinary Meeting, March 20th, 1877.

E. W. Binney, F.R.S., F.G.S., President, in the chair.

"On the Action of Sea-Water upon Lead and Copper," by William H. Watson, F.C.S., communicated by Dr. R. Angus Smith, F.R.S.

While numerous experiments have been made and published from time to time as to the action of distilled water and various saline solutions upon lead and copper, so far as I know very few have been published as to the action of *sea-water* upon them, hence my concluding to record the following:—

The only published researches I have been able to find bearing upon this question are those by Dr. Crace-Calvert and R. Johnson, "On the Action of Sea-Water upon certain Metals and Alloys" (Manchester Lit. and Phil. Soc., 1865), and later those by Kaiser, "On the Action of Sea-Water on Lead" (Arch. Pharm. (3), vi, 405), from which he says that after exposing strips of lead to sea-water for four days no lead could be detected in the water. He, however, does not mention the quantity of metal or the quantity of water used in the experiment, neither does he mention the method of testing.

The experiments of Calvert and Johnson were conducted as follows:—20 square centimetres of each metal, cleaned with great care, were taken and placed in separate glass vessels, immersed in equal volumes of sea-water. After one month the plates were taken out, and any compounds that had adhered to the surface carefully removed; the plates were then dried and reweighed—*the loss being estimated and taken as the quantity of lead dissolved.* To render the results obtained of more practical value, calculations were made, and the following table constructed by them, showing the amount of each metal dissolved by 100 litres of sea-water by acting upon one square metre of each metal :—

	Grammes.
Steel	29·16
Iron	27·37
Copper (best selected)	12·96
„ (rough cake)	13·85
Zinc	5·66
Galvanised Iron	1·12
Block Tin	1·45
Stream Tin	1·45
Lead (virgin)	Trace
„ (common)	Trace

With regard to these experiments we notice that the method adopted for the determination of the amount of metal dissolved was faulty—one by which small quantities could not be with certainty detected. This fact, although taking from the results some of their scientific value, does not deprive them of much practical interest.

The colorimetric method, based upon the depth of colour produced with lead or copper on addition of ammonium sulphide, being capable of detecting exceedingly minute quantities of these metals in solution, was that adopted in the experiments about to be described, so that I am able to speak of the minute variations in the quantity of metal dissolved according to the length of time during which it was exposed to the water, and the results of my experiments show that the action of sea-water upon lead and copper is exerted chiefly during the first few days of exposure, also—which appeared at first somewhat remarkable—that the quantity of metal in solution, after reaching its maximum, decreases when the metal is still exposed in the water.

The method of procedure may be described as follows :—Thin foil of lead and copper was used, cut into pieces two inches square, and thus exposing eight square inches of surface. In the instance of the copper-foil it was effectually cleaned by immersing for a sufficient time in moderately dilute nitric acid, and afterwards washing in water. The lead was cleaned and made perfectly bright by rapidly rubbing with the back of a knife. The pieces were cleaned shortly before being immersed in the sea-water, and until ready for such immersion were kept covered by pure water. The quantities of sea-water (2,000 grain measures in each instance) were poured into beakers, and one of the pieces of foil, copper or lead as the case might be, was dropped into each. The beakers were then placed in a room above the laboratory, where they were covered by pieces of porous paper. The foil was occasionally moved about during the exposure.

I. *Lead.*—Even after the longest exposure—32 days—no insoluble compound was suspended in the water or precipitated to the bottom of the beaker, but the surface of the foil was slightly coated with a deposit; it was tarnished. The standard solution of lead from which the comparison solutions were constructed was prepared by dissolving one grain of the lead foil in as small a quantity of nitric acid as possible and diluting with water to 2000 measures: each 100 measures being then equal to 0·05 of a grain of lead. The following results were obtained:—

Days exposed.	Lead in water.
4	0·000862509 grains.
7	0·00120750 "
14	0·00138000 "
32 (experiment 1)	0·00093875 "
32 (experiment 2)	0·00086250 "

These results show that from the 14th day of exposure to the 32nd the quantity of lead in solution diminished (according to experiment 2) from 0·00138 to 0·0008625 of a grain, a quantity exceedingly small, but sufficient to distinctly show a diminution, and easily detected by the method adopted, in the quantity of water used—2000 grains.

I should explain that the determinations as above were made upon the contents of separate beakers, and in each case on the day the lead foil was removed from the water, so that they were not all made on the same occasion. I felt it desirable to execute a fresh series of experiments in order to prove the accuracy, or otherwise, of the conclusion with regard to the amount of lead in solution diminishing on the foil and water being exposed for a lengthened period. Beakers were prepared with water and lead-foil (sea-water of course) and exposed; but instead of determing the *quantity* of lead in solution, the lead-foil was removed after the same lapse of time as in the former experiments just described, namely, after 4, 7, 14, and 32 days' exposure respectively, but the water from each exposure was kept and the whole of the water samples tested with ammonium sulphide at the same time, and thus by comparing the several depths of colour produced by the various samples comparative results were obtained; these confirming the former ones and supporting the conclusion that the amount of lead in solution in the water diminishes on prolonged exposure along with the metallic surface. The same fact may be applied in the case of copper, as will be presently shown.

The action of sea-water upon lead is very slight, and by another experiment (by immersing in a fresh portion of sea-water a piece of lead-foil used in the former experiments, which had become tarnished, and testing the water after some days' exposure) it appears that no perceptible action is exerted by sea-water upon such tarnished metal.

II. *Copper.*—The experiments about to be described were conducted similarly to those on lead, and, therefore, upon that point I need not dwell. By the action of the sea-water upon the copper a green deposit insoluble in water (or nearly so), but exceedingly soluble in dilute acids was formed, and this contained, as will be presently shown, the chief portion of the copper dissolved. This deposit was somewhat rapidly formed at the commencement of the exposure (some being observed at the bottoms of the beakers even after only 12 hours' exposure); but, after a time, this green compound apparently ceased to form, giving place to a thin but not easily removable covering of a bronze colour on the surface of the foil, on which the water ceased to act; on removing this tarnished foil, however, to some fresh sea-water, action again commenced, resulting as before in the formation of a green-coloured deposit. So as to find whether the bronze covering given to the copper by the sea-water prevented or not the action of the waters upon the copper, one of the pieces of tarnished foil was immersed in 2,000 grain measures of soft river water, and into a similar quantity of the same water a piece of *bright* copper-foil of the same size was placed. After the expiration of five days the water was examined. It had acted considerably upon the *bright* copper, but only slightly on the tarnished,—the copper dissolved in one case being about half as much as in the other. The tarnished surface, then, does not prevent totally the action of soft water upon the copper; or the tarnish is itself acted upon.

Before testing, in each case the green compound was separated from the water, so that the amount of copper might be determined in each separately. The following results were obtained:—

(*a*) Copper in solution in the water.

Days Exposed.	Copper.
4	0·00675 of a grain.
7	0·00350 „
14	0·00300 „
32	0·00300 „

Here we have an instance of the copper dissolved reaching its maximum, then between the 4th day and the 7th day diminishing from 0·00675 to 0·00350; but, as will be seen below, the green compound continued to form.

(b) Copper in the green deposit:—

In order to determine the amount of copper in the compound, it was dissolved in a very small quantity of dilute nitric acid, and the solution thus obtained diluted with water to 4,000 measures. 100 measures of this solution were then taken and diluted to 1,000 measures, to which ammonium sulphide was added, and the tint compared with that produced similarly in a standard copper solution,—the results thus obtained being multiplied by 40 furnish the quantity of copper in the whole of the deposit as follows:—

Days Exposed.	Copper.
4	0·200 of a grain.
7	0·320 „
14	0·360 „
32	0·370 „

Adding the quantities of copper found in solution in the water to those found in the deposit, we obtain the following numbers:—

Days Exposed.	Copper.
4	0·20675 of a grain.
7	0·32350 „
14	0·36300 „
32	0·37000 „

As to the green deposit, I found it to consist chiefly of a chloride of copper, but I am at present unable to say what particular chloride, and as to its formation, I may add that by keeping copper-foil immersed in a solution of sodium chloride containing 53 grains in 2,000 of water (about the quantity present in sea-water), no such deposit is formed neither is it formed when copper-foil is kept immersed for some time in a solution of magnesium chloride.

Having made calculations from results given above, I append tables showing the action of one gallon (70,000 grains) of sea-water upon 280 square inches of each metal. The quantities are expressed in grains.

I.—LEAD.

Days Exposed.	Lead in Solution.
4	0·0301875
7	0·0422625
14	0·0483000
32 (expt. 1)	0·03285625
32 (expt. 2)	0·0301875

II.—COPPER.

Days Exposed.	Copper in Solution.	Copper in Deposit.	Total Copper Dissolved.
4	0·20625	7·00	7·23625
7	0·12250	11·20	11·32250
14	0·10500	12·60	12·70500
32	0·10500	12·95	13·05500

Braystones, near Whitehaven, March 14th, 1877.

Postcript. March 17th, 1877.

Since writing my paper, dated March 14th, I have read in the Chemical News (of yesterday's date, March 16th), page 110, a paper by Mr. M. M. Pattison Muir, "On the Action of Water and dilute Saline Solutions upon Lead" (read before the Manchester Literary and Philosophical Society, Feb. 6th.) I find from this that he has observed, while experimenting on the action of solutions of potassium carbonate, potassium nitrate, and ammonium nitrate upon lead, that the quantity of lead in solution *decreases* after a certain point has been reached. This is similar to the results obtained by me in the case of sea-water. As will be observed, my experiments also show that this peculiar fact applies in the instance of the action of sea-water upon *copper*. In the experiments with sea-water, however, the results mentioned could not be due to the same salts as used by Mr. Muir, as sea-water does not contain them.

"Note on the Upper Coal Measures of Canobie, Dumfriesshire," by E. W. BINNEY, President, F.R.S., &c.

Mr. D. C. Davies, F.G.S., in a very interesting paper on the upper carboniferous strata of Shropshire and Denbighshire, just published in No. 129 of the Quarterly Journal of the Geological Society, in alluding to a section at Canobie of his (the President), printed in vol. ii., third series, of this Society's Transactions, p. 31, states, "Section 2 is one given by Mr. Binney of the strata at Canobie. Mr. Binney describes the upper coal measures, group 1, as the highest in the kingdom. He considers the permians, to begin with the lowest breccia in group 2. These breccia are made up of carboniferous gritstones and limestones. They are interstratified with red shales containing rootlets of *Stigmaria ficoides*, and also with a bed of limestone, together with brown sandstones." Now it is quite true that he considered the permians in this district begin with the lowest breccia in group 2, but the author has no doubt quite unintentionally made a mistake in supposing that the breccias are interstratified with red shales containing rootlets of *Stigmaria*. Such red shales are seen in the river, a little to the north of Canobie Bridge, and lie *under* the lowest bed of breccia, and not interstratified with the breccia beds.

The words he (the President) used at page 351 of his paper are "Under this breccia (the lowest bed) was a bed of red shales, containing rootlets of *Stigmaria ficoides*. I did not see these shales absolutely passing into the breccia, owing to a covering of five yards of fallen bank, but they appear to dip in the same direction, namely, slightly east of south, although at a somewhat less inclination. With these shales I consider the coal measures to commence and the permian strata to terminate at the lowest bed of breccia." Again, at page 352, he states, "The shales under the last bed of breccia, containing *Stigmaria* rootlets, are considered by me to be the highest coal measures ever yet noticed in Great Britain. Probably the passage of carboniferous into the overlying permian beds is more apparent than real, and the bed of breccia doubtless shows a period of disturbance, but in the whole course of my observations, extending over thirty years, I have never seen anything before which appeared so nearly to prove the passage of the one into the other as this section does."

Since the above was written unfortunately nothing more has been done to his knowledge to prove the section. The 600 feet of coal measures lying above the *Spirorbis* limestone are the highest carboniferous strata yet met with, but they have not yet been clearly proved to pass upwards into the permians. This remains to be done.

A short time ago he had again visited the Canobie section in company with his friend Mr. J. W. Kirkby, and in addition to the *Spirorbis carbonarius* formerly found in the limestone, that gentleman recognised in the shales near it *Athracomya?* *Beyrichia arcuata* (Bean), *Spirorbis helictres* (Salter), and a single specimen of a long entomostracan not to be determined from its state of preservation.

It has been stated that the occurrence of the *Spirorbis carbonarius*, which is well known as being found in carboniferous rocks of all ages, from the Bardiehouse limestone to the Ardwick beds, would by itself have little value in identifying strata 200 or 300 miles apart. It was formerly attempted by a very eminent geologist to prove that the Bardiehouse limestone occupied a position above the millstone grit, simply because it contained the *Spirorbis* and some other common coal fossils. No doubt the diagnostic value of such fossils has considerably diminished at the present day; but in addition to containing the *Spirorbis*, this limestone on breaking exhibits a singular porcelain-like appearance, a semiconchoidal fracture, and a mottled character, which are not recognisable in other carboniferous limestones so far as his experience went; and it is always found in the upper part of the coal measures near to permian beds, and generally makes a good hydraulic lime. In addition, it was associated with purple and greenish-coloured shales and fire clays as well as green earth, and what has sometimes the appearance of volcanic ash. Altogether the general character of these strata, independently of the occurrence of the *Spirorbis* in the limestone, give evidence of coal measures very different in appearance to those found in the middle and thick beds of the English and Scotch fields.

Many years since the late Sir Charles Lyell showed him a specimen of the limestone and asked what was its age, as none of his geological friends knew it. He (the President) explained that it was the so-called Freshwater limestone of the upper coal measures. Certainly *Spirorbis* is a much better specific name for the limestone than Freshwater. A good many geologists and mining men even now on being shown a specimen of the limestone would be puzzled to know where it came from.

"Losses and Gains in the Death-toll of England and Wales during the last 30 years," by ARTHUR RANSOME, M.A., M.D.

In a paper recently read before this Society, Mr. Baxendell[*] endeavours, with much research and ability, to reckon up the gains and losses in the mortality of England and Wales from different diseases. If the records upon which he bases his conclusions had been entirely trustworthy, the results which he has obtained would doubtless have been valuable—and might, perhaps, have indicated the presence of certain influences bearing upon the public health—but even so, I venture to think that the figures he adduces would not bear out the inferences which he attempts to draw from them. Unfortunately, the returns of the Registrar-General

"On Changes in the Rates of Mortality from Different Diseases during the Twenty urs 1854-1878."

cannot be relied upon for the species of information which he seeks to obtain from them.

Even when it is required only to estimate the gross mortality of various places, there are important limitations to their usefulness, owing to the various nature of the boundaries of the several registration districts, the presence or absence of hospitals, and the movements or relative composition as to age, &c., of the populations. Carefully collated data on these several points are given in the later reports, and by dint of much labour some of the more serious errors from these causes may be avoided, but when we come to the nosology of the tables and try to pick out, as Mr. Baxendell has done, the causes of the mortality in different years, we are at once foiled of accurate results by the imperfections of the records and the impossibility of verifying the causes of death.

These drawbacks to the utility of the returns have often been pointed out, and in the early reports of the Registrar-General, Dr. Farr himself complains of the imperfect nomenclature of disease. Doubtless some improvement has taken place of late years, but such variations in the accuracy of diagnosis at once prevent any value from attaching to a comparison between earlier and later records. Until a very recent period the registration of deaths was not compulsory at all; and in the Report of the Medical Officer to the Privy Council for the year 1864, it is noted that out of 500 deaths registered in St. David's district only 15 were recorded by properly qualified medical practitioners. To the present time it is highly probable that the disease Phthisis or Consumption is far too often recorded as the cause of death in some districts in Wales, since it is made to include many forms of disease that cause wasting before they cause death.

Dr. Haviland's recent map of the distribution of phthisis, thus makes it appear that the Island of Anglesey is one of the worst districts in the kingdom for the production of this disease—the great probability being that it is no worse than its neighbours.

It is most likely owing to this imperfection of nomenclature that changes have taken place in the apparent rates of mortality from some of the diseases mentioned by Mr. Baxendell. Many of the deaths recorded of late years as arising from bronchitis, would probably have been set down in former times to pneumonia; and hence the former disease appears to have increased in fatality, the latter to have diminished. Hence, also, owing to an improved diagnosis the term dropsy will have given place to the name of the organ out of whose derangement it has arisen; and we have it less frequently recorded as the cause of death, whilst diseases of the heart, liver, and kidneys have apparently increased in prevalence.

The vague terms brain disease, cephalitis, and convulsions, may also have undergone some amount of transposition.

But Mr. Baxendell points out a very great increase (86 per cent) in one disease—small-pox—that is not likely to have been mistaken for anything else; and he further shows that this increase has taken place mainly amongst the adult members of the population.

His remarks on this point are undoubtedly very important and will need to be well weighed by those who are responsible for the efficient performance of vaccination.

It is important to observe, however, that the whole of this increase occurred in the years 1871 and 1872, when over 40,000 deaths took place from this cause.

The period of ten years chosen by Mr. Baxendell is probably not long enough to afford a basis of argument respecting epidemic diseases—40 or 50 years are sometimes required to contain the full cycle observed by some of them—as may be seen from the diagram showing the variations in the prevalence of Scarlet Fever in Sweden.

The cycle usually followed by small-pox is, however, shorter than this, and from the exemption of children from the excessive death rate by this disease, it would seem as though the protection afforded by infantile vaccination endures now for a shorter period than it formerly did.

This surmise is in accordance with the facts recorded by Mr. Simon respecting the increase in the number of cases of successful re-vaccination in the Prussian army during the years 1813 to 1833.

It points to some imperfection in the operation or to a possible deterioration in the quality of the lymph, owing to its transmission too constantly through the human subject. In any case, its cause is worthy of full and patient investigation.

The objections that I have ventured to bring against the details of Mr. Baxendell's tables, also do not apply to his comparison of the gross mortalities of the two periods he has chosen.

The almost stationary condition of the death-rate of the country during the last thirty years has been noticed by many who are interested in the public health, and who have been disappointed at not obtaining from the Registrar-General tables of total mortality, that proof they so much desired of the efficacy of their sanitary improvements. Mr. Baxendell, indeed, shows that all the zymotic class of diseases, with the single exception of small-pox, have somewhat diminished; and by limiting the term 'preventible' to this class of disorders he is able to ascribe to sanitary measures the modicum of improvement represented by "a saving of one life in every 134," or "a reduction of 0·17 per cent. in the general death-rate."

At the risk of sacrificing even this small tribute to the value of sanitary work, I must, however, point out that this restriction of the term 'preventible' is not usually recognised by the medical profession. The sanitarian claims much more than epidemic diseases as truly preventible. The influence of favourable or unfavourable sanitary conditions is also plainly shown in such diseases as phthisis, diarrhœa, and dysentery, scrofula, various pulmonary complaints, convulsive disorders, and, to some extent, in the whole range of diseases that produce our excessive infant mortality. The true meaning of the term 'preventible' mortality is fully given by Mr. Simon in his preface to Dr. Greenhow's Inquiry. Briefly stated it means "all local excuses of fatality that are due to local circumstances of aggravation." (p. viii.)

The excessive mortality from a large group of diseases other than epidemic in their character may then be greatly diminished, partly by public sanitary work and still more by improvements in domestic hygiene amongst the poor—such as sanitary associations and medical officers of health may by degrees be able gradually to teach.

It may fairly be asked then of sanitarians, where is the proof of your ministry?

The range of preventible mortality is greater than Mr. Baxendell supposes, but it has not been prevented—to what must this result be attributed? Mr. Baxendell seems inclined to question the utility of the costly sanitary works that have been undertaken of late years; but an examination of the local details that make up the total mortality will soon show that, in places where these works have been thoroughly carried out, a very distinct effect upon the mortality has been produced, and that the stationary condition of the total death-rate is mainly caused by the increasing mortality in places that have not been thus cared for.

It is indeed difficult for anyone acquainted with the history of the science of public health to see the necessity for any defence of sanitary works—seeing that in some form or other they have led to the entire disappearance of some diseases, and to the great amelioration of others—and that where they have been thoroughly carried out they have had a distinct effect, either in directly diminishing the death-rate or in counteracting a tendency to increased disease and death.

The Registrar-General in his last report gives a few examples of places in which there seems to have been a direct connection between sanitary improvements and diminished rates of mortality. His list is as follows; but the time at my disposal does not permit me to append his remarks as to the kind of works executed at each place :—

Name of District.	County.	Population.		Average Annual Mortality		
		1861.	1871.	1841-50.	1851-60.	1861-70.
North Witchford	Cambridge..	14,791	15,585	27	21	20
Whittlesey	,, ...	6,966	7,002	25	23	21
Wisbeach	,, ...	33,309	34,209	25	22	20
Ossett..............	Essex	11,595	13,172	24	21	18
Salisbury	Wilts.........	9,039	9,212	28	24	20
Stoke Damerel...	Devon	50,440	49,449	26	23	21
Wolverhampton..	Stafford ...	126,902	136,053	27	28	24
Coventry	Warwick ...	41,647	40,113	27	25	21
Macclesfield	Chester......	61,543	59,339	26	25	23
Hull	York	56,888	68,316	31	25	26
Newport...........	Monmouth..	51,412	61,252	24	22	21
Merthyr Tydfil...	Glamorgan..	93,008	104,239	28	29	25
Crickhowell	Brecknock..	22,457	20,147	27	25	23

But it may be said that of late some form of sanitary administration has been at work all over the country, why then have recourse to detached examples? The reply is sufficiently simple, that this universal sanitation has certainly not yet had time to prove its power for good.

I should be very unwilling to defend the present chaotic, inefficient, and extravagant method of carrying on public health government: under it the local medical officers have no security of office, many are appointed over petty areas upon paltry salaries, the aggregate of which would produce a good income for well trained men. Most of them have had no special training, and none of them have any means of proving their capacity by a state qualification—they have no well organised system for obtaining immediate information of the outbreak of disease—and from the want of proper instruction and central assistance they are driven to make experiments in sanitation which by their failure or by their cost raise a formidable local opposition to all their efforts.

It will be seen then that I entirely agree with Mr. Baxendell in thinking that "the present sanitary system is very defective"—but it by no means follows that it has done no good.

In auditing carefully the several items of a profit and loss account, there may well be found an equipoise between the two sides of the account—or even a loss on the whole—and yet it may be possible to see that unless certain adverse circumstances had occurred, such as a rise in wages, increased price of raw material, &c., the result would have been a decided gain.

The general death-toll of a country is in truth under the influence of many most various conditions. It is only possible to discover the full effect of sanitary measures when all these conditions are taken into account. It would be a weak form of defence for these measures if we were to say that if they had not been taken "things might have been worse,"—for they might, perhaps, even have been better.

But the argument changes its character altogether if it can be shown that the death rate certainly would have been heavier had not sanitary or other influences been active to counteract the increase of morbific agencies that have been at work.

It is not difficult to prove that certain important causes of excessive mortality have of late years increased considerably in their pressure upon the populations of England and Wales; and I would especially advert to three of these:—1. The density and size of towns; 2. Intemperance; 3. The improper feeding and treatment of children.

1. *The Density and size of Towns.*

"In the last twenty years," says Dr. Farr, "the towns of England have increased from five hundred and eighty to nine hundred and thirty-eight, their population from nine to fourteen millions." Supplement to 35th Ann. Rep., p. viii.

The population throughout the country has increased so that, on the average, there are now 1·74 acres to each inhabitant, and in the decade (1841—50), there were 2·21—and in most of the towns there is a still greater proportionate increase in the density; thus, in London, it has doubled the acreage to a person; sinking from ·04 to ·02, and the same proportionate increase is found at Plymouth (·04 to ·02); at East Stonehouse it has sunk from ·02 to ·01; in Salford and Portsmouth the space has diminished from ·08 to ·05; in Leicester from ·07 to ·04; in Stoke-on-Trent from ·23 to ·15; in West Bromwich from ·32 to ·19.

It is not easy to estimate the exact effect upon health of this crowding together of populations. Its operation is compounded of many distinct elements; thus Dr. Farr himself ascribes a direct influence to the dead and decomposing organic matter floating in the air of towns, the product of respiration and of excrementitious matter of all kinds; and there can be little doubt that it is a powerful predisposing cause of the large group of scrofulous diseases that annually carry off more than 30,000 persons.

But in towns there are found many other malign influences in full operation in much greater abundance than in the country; ill-ventilated, unhealthy, crowded dwellings; sedentary and unwholesome trades and occupations; together with much freer play to the two other causes of increased mortality that have been mentioned, such as increased facilities and temptations to the use of strong drink; female employment, and consequent neglect of children by their mothers.

That town-life is more unhealthy than country-life, and that the mortality increases in some way with the density of the population is sufficiently shown by the following table:

Mean Annual Death-Ratio.	Density.	Births.	Deaths.	Annual Increase of Population.
	Persons to a Square Mile.			
15 39	315	35·11	22·00	11·99
15 17	166	30·22	16·75	14·69
18 20	186	32·19	19·16	7·53
21 23	379	35·78	21·88	13·82
24 26	1718	38·75	24·90	18·59
27 30	4499	40·16	28·08	13·28
32 (Manchester)	12·375	37·33	32·49	3·22
39 (Liverpool)	65·823	37·57	38·62	— 12·33

The law of increased mortality with increased density of population is, on the large scale, so definite and certain that Dr. Farr has been able to construct a formula by which in many cases the mortality can be calculated from the density—and if accurate data were obtainable it might even be possible to ascertain what increase of mortality might have been expected to result from the increased tendency of populations to aggregate themselves together. We cannot state the problem quite so definitely as this would imply, but if we take Dr. Farr's estimate of 5 millions of additional inhabitants in our towns, and apply to this the usual urban rate of 24 as distinguished from the rural rate of 19 per 1,000, we obtain an excess of 25,000 deaths that would have to be placed to the account of overcrowding.

The fact that, on the whole, the mortality has remained stationary during the last 30 years, shows that 25,000 lives must have been saved by other means, and hence that some influences have been at work to mitigate the evil effects of overcrowding. What these influences are is made at least very probable by the fact that, in most of the notable exceptions to Dr. Farr's law, sanitary improvements have been carried out for a good many years. In the following examples it will be noted that the density of most of the places named has decidedly increased whilst the mortality has diminished.

The first 6 of these districts are taken from the table already quoted from the Registrar General as having improved as the result of sanitary works, and in each of the remainder it will be found that similar works have for some time been pursued.

Districts.	Density, Acres to a Person.			Death-rates.		
	1841-50.	1851-60.	1861-70.	1841-50.	1851-60.	1861-70.
Salisbury	·05	·05	·05	28	24	20
Wolverhampton	·58	...	·42	27	28	24
Coventry	·16	...	·13	27	25	21
Macclesfield	1·37	1·31	1·36	26	25	23
Newport	2·88	...	1·96	24	22	21
Merthyr Tydfil	1·74	...	0·71	28	29	25
Gloucester	1·09	·96	·84	24	22	22
Leicester	·07	·06	·04	27	25	26
Clifton	·41	·34	·26	23	20	20
Salford	·08	·06	·05	28	26	27
Cheltenham	·59	·53	·50	20	19	19
Altrincham	2·26	1·98	1·63	23	23	21
Newport	3·13	3·06	3·03	20	20	18
Dover	1·13	1·00	·87	21	20	20

2. *Intemperance* has been assigned as an increasing cause of mortality during the last 30 years. There is no doubt as to the increased consumption of alcoholic drinks of late years. "In the five years ending 1845 the average expenditure of the United Kingdom upon these drinks was about 77 millions sterling, whilst in the five years ending 1875 it was 135 millions." (W. Hoyle.)

It is difficult to learn how much of this alcohol has been taken in excessive quantities, and how much has been added in small amounts to the diet of those who formerly could not afford to take any. Many persons doubtless would come under the last-named category. During the last 20 years trade has on the whole improved. There have been higher wages paid, and a portion of these will have been spent merely in dietetic drinking, but it is certain that there has been a large increase in the numbers of those who drink to intoxication. "In 1860 the apprehensions for drunkenness were only 88,000, in 1875 they were 203,000." (W. Hoyle.)

This increased intemperance must have shortened many lives—to repeat here Mr. Neison's table—a temperate person's chance of living as against that of an intemperate person—is at the age of 20 years, 44·2 years to 15·6 years; at 30, 36·5 years to 13·8 years; at 40, 28·8 years to 11·6 years; at 50, 21·25 years to 10·8 years; at 60, 14·28 years to 8·9 years. In intemperate persons, the mortality at 21 to 30 years of age is 5 times that of the temperate; from 30 to 40 it is 4 times as great.

Since alcohol affects chiefly the liver, kidneys and heart, we may with much probability, assign the great increase in diseases of these organs, pointed out by Mr. Baxendell, mainly to this cause, and not to defective sanitary administration.

It is of course impossible to say how much of the mortality of the country is due to this cause; but I will mention one instance in which it would appear that some approximation may be made. My friend Dr. W. Roberts informs me that about 30 years ago a very powerful temperance movement affected large districts of Wales to such an extent that in Anglesey most of the public houses were closed and a large proportion of the people became total abstainers from alcohol.

This movement had to a great extent died out by the year 1850.

Now, the density of the population in this county (of Anglesey) has remained almost stationary, but its mortality has increased, as shown by the following table:

DENSITY AND MORTALITY IN THE ISLAND OF ANGLESEY IN THREE DECENNIADS.

District.	Density.			Annual Mortality.		
	Persons to a Square Mile.			Deaths to 1,000 Living.		
	1841-50	1851-60	1861-70	1841-50	1851-60	1861-70
Anglesey........	3·41	3·16	3·36	17	19	21

Since the population of these districts has certainly improved in material comfort in the last 20 years, it is probable that a large part of the increased mortality may be fairly ascribed to the increased use of alcohol. A similar statement holds good with regard to other parts of Wales.

If it were allowable to extend the analogy of these places to the whole of England, the stationary condition of the death-rate of the country would have to be accounted for by discovering some causes that had diminished the death-rate by at least two or three per 1,000, and had saved about 40,000 or 50,000 lives every year.

I cannot help thinking that in many parts of England this statement falls short of the truth, and that we have to thank sanitary effort for saving the country from a still greater loss.

3. The third element in the positive increase of the death-rate is the *infant mortality.* Now, this I have already included to some extent under the head of 'preventible mortality;' but whilst it is, perhaps, more under the control of proper hygienic conditions than any other forms of death,

these conditions are rather to be ranged under the sub-title of domestic arrangements than as pertaining to the ordinary sanitary work of local authorities. It is less likely than adult mortality to be at once affected by the more common mechanical improvements of districts, such as improved sewage and good water supply; and it requires for its amelioration better house accommodation, more efficient house drainage, a freer supply of pure air, cleanliness, a proper supply of suitable sustenance, and adequate protection against infection.

It cannot be said that these points have yet received full attention from the sanitary administrators of the country, and hence the death register of infants can hardly be quoted as evidence of their failure.

On the other hand, children are more exposed than adults to the already mentioned causes of ill-health; they are more directly affected by over-crowding, and they are the first to feel the effects of the neglect arising from intemperance. This indirect consequence of the abuse of alcohol is probably still more fatal than its direct influence.

It is probable that the mode of feeding young children, especially in the manufacturing districts, is becoming less and less adequate to their needs.

The inquiries made by Dr. Ferguson of Bolton, and others, show that tea and many other forms of experimental foods are now tried upon children, instead of the milk that was formerly more easily procured, and that the result is a gradual falling off in the physical power of our manufacturing populations.

There can then be no doubt that the adverse iufluences bearing upon infant life have been increasing in intensity during the last 20 years, and they have not failed to produce their effect.

Notwithstanding the effects of overcrowding and intemperance, the adult mortality of the country, owing to some beneficent cause or other, has on the whole slightly diminished, but the mortality under 5 years of age has increased.

The following table, kindly prepared for me by Mr. Royston, from the returns of the Registrar General, shows this in a striking manner—the adult mortality having lessened ·17 per 1000, the infant mortality having increased 1·50 per 1000 in the 15 years ending 1870.

MORTALITY IN ENGLAND AND WALES.

Years.	Total Mortality per 1000 of Population.		Mortality under 5 to Population at same ages.	
1838 to 1840	22·4	Average of 18 years 22·47	66·55	Average of 18 years 66·68
1841 to 1845	21·4		63·9	
1846 to 1850	23·35		67·0	
1851 to 1855	22·7		69·15	
1856 to 1860	21·85	Average of 15 years 22·3	67·25	Average of 15 years 68·16
1861 to 1865	22·6		69·05	
1866 to 1870	22·45		68·2	

I think it will be acknowledged that it is not easy for sanitary science to struggle against the hostile forces that have now been enumerated. If they had not been met by measures to some extent competent to mitigate their power the condition of the country would have been perilous in the extreme, and at the present time we are by no means free from serious danger owing to their increasing pressure —danger of epidemic disease fostered by the organic matter generated by overcrowding—danger from intemperance to both national health and national wealth—and danger of deterioration of race owing to the sickliness and stunted growth of the children that survive. It is surely our duty then to promote a more efficient system of sanitary administration—to take full advantage of the facilities afforded by recent legislation for the improvement of our towns—to try to abate the practice of intemperance—and to endeavour to spread amongst all classes a knowledge of the laws of health.

Ordinary Meeting, April 3rd, 1877.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

"On the Action of Water and Dilute Saline Solutions upon Lead."—Addendum to Part III.—By **M. M. Pattison Muir**, F.R.S.E., Assistant Lecturer on Chemistry, the Owens College.

In a paper read to the Society (Proc. xvi., 142) I recorded the results of a few experiments upon the effect of rolling sheet lead as tending to alter the solvent action of dilute saline solutions upon such lead. I have now to supplement these experiments by others of a similar description. The results are tabulated below.

Sample No. 1.

EXPERIMENTS CARRIED OUT IN CORKED FLASKS, 500 cbc. LIQUID IN EACH.

Salt.	Grams per litre.	Surface of Lead. sq.cm.	Nature of Lead.	Lead dissolved in Mgms. after		
				2 days	5 days	16 days
Water	—	25	As purchased	0·7	trace	trace
Potassium Carbonate	0·20	,,	,,	trace	none	none
Do. Nitrate	,,	,,	,,	1·7	trace	trace
Ammonium do.	,,	,,	,,	1·7	1·1	10·0
Do. Sulphate	,,	,,	,,	trace	trace	trace
Calcium Chloride	,,	,,	,,	trace	0·5	1·0
Water	—	,,	Rolled out till thinner	trace	0·8	0·5
Potassium Carbonate	,,	,,	,, ,,	trace	trace	trace
Do. Nitrate	,,	,,	,, ,,	1·3	11·0	14·0
Ammonium do.	,,	,,	,, ,,	1·3	6·0	10·0
Do. Sulphate	,,	,,	,, ,,	1·0	0·7	0·4
Calcium Chloride	,,	,,	,, ,,	trace	trace	0·5
Water	—	,,	Rolled out till *much* thinner	1·0	0·5	0·3
Potassium Carbonate	,,	,,	,, ,, ,,	trace	trace	trace
Do. Nitrate	,,	,,	,, ,, ,,	0·7	0·3	trace
Ammonium do.	,,	,,	,, ,, ,,	1·5	1·7	10·0
Do. Sulphate	,,	,,	,, ,, ,,	1·0	0·8	0·4
Calcium Chloride	,,	,,	,, ,, ,,	trace	trace	0·5

EXPERIMENTS CARRIED OUT IN BEAKERS, LOOSELY COVERED WITH POROUS PAPER, 500 cbc. LIQUID IN EACH.

Water	—	25	As purchased	1·0	0·2	trace
Potassium Carbonate	0·20	„	„	trace	trace	none
Do. Nitrate	„	„	„	0·3	trace	trace
Ammonium do.	„	„	„	1·2	0·5	trace
Do. Sulphate	„	„	„	1·0	0·8	0·5
Calcium Chloride	„	„	„	1·0	0·8	0·7
Water	—	„	Rolled out till thinner	0·8	0·4	trace
Potassium Carbonate	„	„	„ „	trace	trace	none
Do. Nitrate	„	„	„ „	trace	trace	trace
Ammonium do.	„	„	„ „	1·0	1·0	0·8
Do. Sulphate	„	„	„ „	1·0	0·6	1·0
Calcium Chloride	„	„	„ „	0·3	0·3	1·0
Water	—	„	Rolled out till *much* thinner	0·4	trace	trace
Potassium Carbonate	„	„	„ „ „	none	none	none
Do. Nitrate	„	„	„ „ „	0·3	trace	trace
Ammonium do.	„	„	„ „ „	1·2	0·4	trace
Do. Sulphate	„	„	„ „ „	1·0	0·8	1·0
Calcium Chloride	„	„	„ „ „	0·3	0·3	2·0

Sample No. 2.

EXPERIMENTS CARRIED OUT IN CORKED FLASKS, 500 cbc. LIQUID IN EACH.

Water	—	25	As purchased	0·5	0·4	0·4
Potassium Carbonate	0·20	„	„	trace	none	none
Do. Nitrate	„	„	„	0·3	0·3	0·3
Ammonium do.	„	„	„	1·1	0·5	0·4
Do. Sulphate	„	„	„	0·4	0·2	0·2
Calcium Chloride	„	„	„	0·5	0·3	0·2
Water	—	„	Rolled out till thinner	0·3	0·5	0·4
Potassium Carbonate	„	„	„ „	trace	trace	trace
Do. Nitrate	„	„	„ „	1·6	20·0	30·0
Ammonium do.	„	„	„ „	0·6	10 0	18·0
Do. Sulphate	„	„	„ „	0·3	0·3	0·3
Calcium Chloride	„	„	„ „	trace	0·3	0·3
Water	—	„	Rolled out till *much* thinner	0·3	0·3	0·3
Potassium Carbonate	„	„	„ „ „	trace	trace	trace
Do. Nitate	„	„	„ „ „	1·0	0·5	0·4
Ammonium do.	„	„	„ „ „	1·5	12·0	25·0
Do. Sulphate	„	„	„ „ „	0·3	0·3	0·3
Calcium Chloride	„	„	„ „ „	0·3	0·3	0·3

EXPERIMENTS CARRIED OUT IN BEAKERS.

Water	—	25	As purchased	0·2	trace	trace
Potassium Carbonate	0·20	„	„	trace	„	„
Do. Nitrate	„	„	„	0·4	„	„
Ammonium do.	„	„	„	0·8	„	„
Do. Sulphate	„	„	„	trace	„	„
Calcium Chloride	„	„	„	trace	„	„
Water	—	„	Rolled out till thinner	0·2	„	„
Potassium Carbonate	„	„	„ „	trace	„	„
Do. Nitrate	„	„	„ „	0·4	„	„
Ammonium do.	„	„	„ „	0·8	„	„
Do. Sulphate	„	„	„ „	trace	„	„
Calcium Chloride	„	„	„ „	trace	„	„
Water	—	„	Rolled out till *much* thinner	0·2	„	„
Potassium Carbonate	„	„	„ „ „	trace	„	„
Do. Nitrate	„	„	„ „ „	0·3	„	„
Ammonium do.	„	„	„ „ „	0·5	„	„
Do. Sulphate	„	„	„ „ „	0·2	„	„
Calcium Chloride	„	„	„ „ „	0·2	„	„

The general result which I deduce from these experiments is that rolling has no material influence upon the solvent action of water and of dilute solutions of Potassium Carbonate, of Ammonium Sulphate, and of Calcium Chloride upon lead; but that rolling tends to increase the solvent action of dilute solutions of Potassium and Ammonium Nitrates upon lead. This conclusion applies only to experiments carried out in corked flasks. In the experiments performed in Beakers the conditions are complicated by the admission of air, and consequent more or less complete precipitation of the soluble lead salt at first produced.

"The Drunkenness Statistics of the Large Towns in England and Wales," by J. H. POYNTING, B.A., B.Sc.

The February number of the *Fortnightly Review* contains an article by Mr. Chamberlain on "Municipal Public Houses," in which he incidentally combats the proposition that "the multiplication of public-houses has a tendency to diminish intemperance." To prove the error of this he has given a diagram in which the 71 towns of England and Wales with a population above 20,000 are arranged in order from Cambridge, with the greatest number of public houses in proportion to the population (one to every 100 persons), to Plymouth with the least (one to every 340 persons).

The following table contains the towns arranged in their order:—

I. Table giving the proportion of public-houses to population in all towns of 20,000 inhabitants and upwards in England and Wales for the year 1875. [From Mr. Chamberlain's article.]

No.*		1 in	No.*		1 in	No.*		1 in
1	Cambridge	100	25	Derby	140	49	Wigan	190
2	Canterbury	100	26	Ipswich	140	50	Rochdale	190
3	Oxford	110	27	Manchester	140	51	Hastings	190
4	Southampton	110	28	Salford	150	52	Great Grimsby	190
5	Shrewsbury	110	29	Chester	150	53	Stalybridge	190
6	Northampton	120	30	Blackburn	150	54	Stockport	190
7	Norwich	120	31	Gravesend	150	55	Oldham	190
8	Portsmouth	120	32	Leamington	150	56	Bolton	200
9	Reading	120	33	Newcastle-on-Tyne	150	57	Liverpool	200
10	Dover	130	34	Sheffield	160	58	Birkenhead	200
11	Swansea	130	35	York	160	59	Devonport	200
12	Great Yarmouth	130	36	Nottingham	160	60	Gateshead	200
13	Worcester	130	37	Lincoln	160	61	Carlisle	210
14	Hanley	130	38	Bath	170	62	Cardiff	220
15	Coventry	130	39	Leicester	170	63	Warrington	230
16	Scarborough	130	40	Sunderland	170	64	Halifax	230
17	Bristol	140	41	Ashton-under-Lyne	170	65	Exeter	230
18	Tynemouth	140	42	Wakefield	170	66	Huddersfield	240
19	Walsall	140	43	Macclesfield	170	67	Leeds	240
20	Brighton	140	44	Preston	180	68	Bradford	260
21	Maidstone	140	45	Hull	180	69	Southport	270
22	Colchester	140	46	South Shields	180	70	Middlesborough	300
23	Newport, Mon.	140	47	Birmingham	180	71	Plymouth	340
24	Wolverhampton	140	48	Dewsbury	180			

* The numbers of the towns are those by which they are denoted in Figs. I., II., III.

Mr. Chamberlain has exhibited the proportion by means of a curve in black ink which I have copied in Fig. I. The numbers are taken from the Blue Book Police (Counties and Boroughs) Reports for the year ending September, 1875. In this figure the towns of Table I. are supposed to be arranged in order from left to right, and the height of the curve opposite each town represents the number of persons to each public-house. Owing to want of space, only every fifth town is actually printed. Mr. Chamberlain then puts a mark opposite each town on the same diagram showing the number of persons proceeded against for drunkenness in proportion to the population in that town during the same year, the numbers varying from one in 500 in Cambridge and Maidstone, to one in 20 in Tynemouth, South Shields, and Liverpool. The marks thus placed are joined by a red ink curve. Mr. Chamberlain remarks that "if the paradox of the supporters of free trade in drink were sustainable by these statistics the red line would incline throughout from right to left in the opposite direction to the black line; but it will be seen that it crosses backwards and forwards with no approach to any order or law."

But it seemed to me on inspecting the diagram that on the average the number of apprehensions for drunkenness increased considerably towards the right hand of the diagram where the public houses were fewest in proportion to the population. To see if this was really the case I grouped the numbers together in the following way. I took the mean of the numbers of persons to each apprehension for drunkenness on the first twenty towns, and put a mark on the diagram opposite the middle town of the twenty at a height representing this mean; then the mean of the twenty towns from the second to the twenty-first, and put a mark opposite the middle town of the twenty to represent this mean; then the mean of the twenty towns from the third to the twenty-second, and so on. Running a curve as nearly as possible through the fifty points thus obtained I found the curve to take the course given in Fig. II. The numbers for the year ending September, 1876, treated in the same way, gave a similar curve, both curves slanting very decidedly in the opposite direction to the public-house curve, and would seem to indicate that the fewer public houses there are the greater on the average the number of apprehensions for drunkenness. This method of examining the numbers seems justified in this respect, that grouping a large number of towns together will to some extent neutralise the effect of more or less stringent police regulations and other circumstances peculiar to individual towns.

It would of course be absurd to infer from these curves that *because* there are more public-houses in certain towns, therefore there is less drunkenness. It would be far more likely that the fewness of public-houses, and the prevalence of drunkenness, arise from some common cause. It seemed possible, for instance, that when there was great prosperity the population would probably increase quickly, and through the difficulty in obtaining licenses, the public-houses would not keep pace with the increase of the population. At the same time, when there was great prosperity, there would be higher wages and more money to spend, and therefore more possibility of drunkenness.

I therefore grouped the towns as before, in twenties, but this time taking the numbers showing the percentage increase in population during the years 1861-71, and then drawing a curve nearly through the points thus obtained, it took the course shown in Fig. III. This, by altering the scale, might almost be made to coincide with the public-house curve in Fig. I.

If the towns be now arranged in order of their percentage increase of population, during these ten years, from Canterbury, Bath, and Ashton, with either no increase or a decrease to Huddersfield, with an increase of 217 per cent, the increase in population can be represented by the curve given in Fig. IV. If the figures for the apprehensions for drunkenness in these towns be grouped by twenties as before, and a curve be run nearly through the points representing the means, Fig. V. is obtained, in which the curve, though not so regular as those of figures 3 and 4, still takes in general the opposite slant to the curve of Fig. IV.

This method of examining the numbers is unsatisfactory in this point, that a town such as Liverpool, with 490,000 inhabitants, has only the same effect on the course of the curve as Southport with 20,000 inhabitants. Fig. VI. represents the proportion of public-houses per ten thousand of the population on this principle. The population of each town is taken as the nearest ten thousand, to save trouble in calculating. The towns are then grouped together in hundred thousands, so that each square now represents, instead of a town, a population of 100,000. The numbers at the top refer to the towns with the same numbers in Fig. I. included in each 100,000. If the numbers of apprehensions for drunkenness for 10,000 of the population in each of these populations of 100,000 be grouped by tens as before, the curve in Fig. VII. is obtained. This does not now follow the opposite course to the public-house curve so regularly as did those of Figs. 3 and 4, though it is still, on the whole, higher on the right hand where the public-house curve is lower.

The two great rises are caused chiefly: the first by Manchester and Salford; the second by Liverpool, as will be seen by referring to the numbers in Fig. VII. opposite the rises.

If the towns be arranged in order of apprehensions for drunkenness for the year 1875, from Cambridge with one in 500 to Liverpool with one in 20, it at once becomes evident, that on the whole, in the north of England, there is a greater proportion of apprehensions than in the south.

The following are the towns arranged in that order:—

TABLE II.

1	Maidstone Cambridge
3	Norwich Devonport
5	Colchester
6	Hastings
7	Ipswich
8	Oxford
9	Portsmouth
10	Dover
11	Brighton
12	Coventry Bath
14	Leamington
15	Canterbury
16	Great Yarmouth
17	Reading
18	Exeter
19	Plymouth Sheffield
21	Leicester
22	Northampton
23	Bristol
24	Lincoln
25	Bradford Birmingham Huddersfield
28	Gravesend Worcester Southampton Macclesfield Preston
33	Leeds Shrewsbury
35	Hull York Walsall
38	Hanley Blackburn Bolton
41	Carlisle Cardiff Wolverhampton Scarborough Swansea
46	Newport Chester Stalybridge
49	Halifax Southport Middlesborough
52	Oldham Wigan Nottingham Derby
56	Wakefield
57	Sunderland Ashton Rochdale Great Grimsby Stockport
62	Manchester Salford Newcastle Dewsbury Birkenhead Gateshead Warrington
69	Tynemouth South Shields Liverpool

If a map of England and Wales be drawn with the parallels of latitude and longitude, and the average numbers of apprehensions per ten thousand in each square formed by these parallels (or where the districts are populous in each half or quarter square) are marked in their respective squares, it will be seen that the drunkenness apprehensions are nearly the same along the south and south-east coasts, and that they then increase in a direction turning round more and more to the north-west, as we proceed northward. I have not had time to examine the numbers for the smaller towns and for the counties to see whether they bear this out.

The numbers from which the curves are constructed are not the exact ones obtained from the police report; usually I have taken the nearest tens to facilitate calculation.

FIG. 1. FIG. 2. FIG. 3.

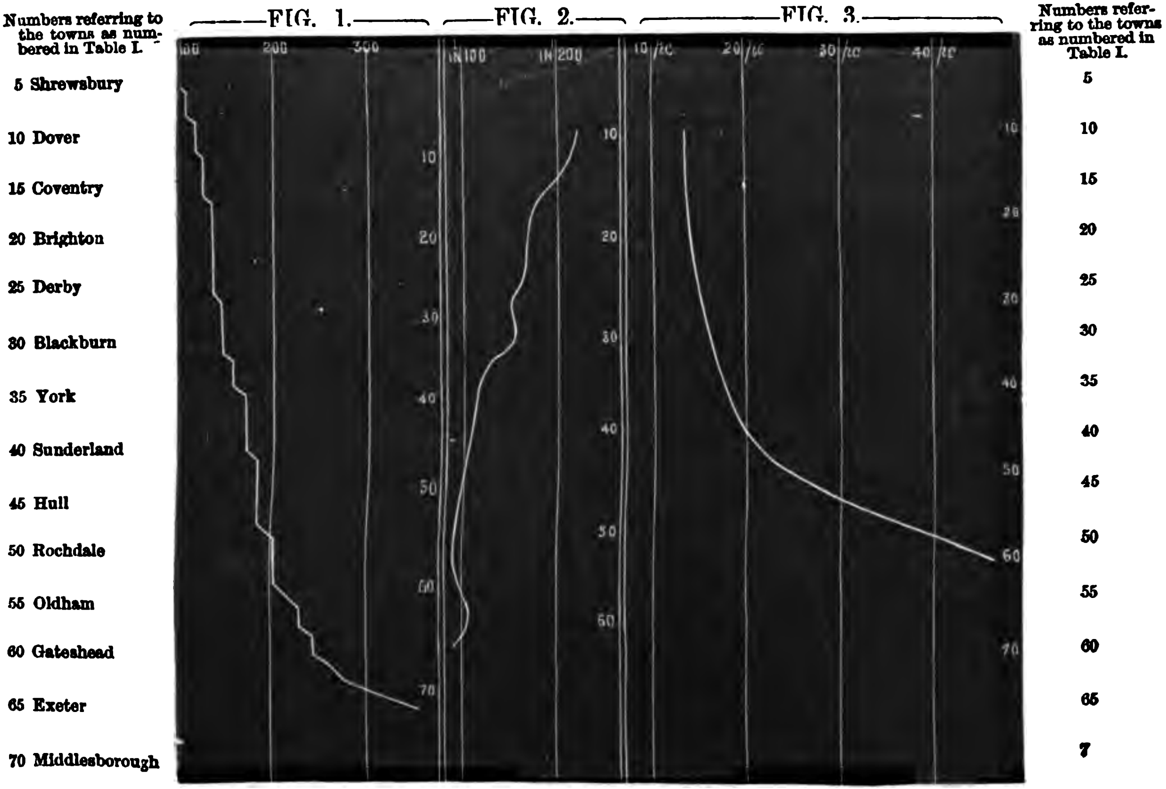

FIG. I.—Table showing the proportion of public-houses to population in all towns of 20,000 and upwards in England and Wales (*Fortnightly Review*, Feb., 1877). Year ending September, 1875.

FIG. II.—Table showing the proportion of persons proceeded against in year ending September 29, 1875.

FIG. III.—Table showing percentage increase of population in the towns from 1861 to 1871. The curve obtained by grouping the towns in twenties.

—FIG. V.— —FIG. IV.—

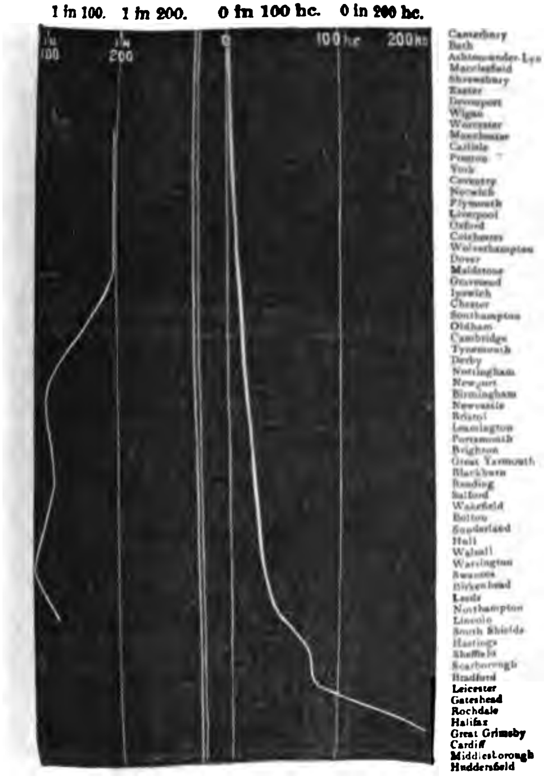

FIG. IV.—Table showing the percentage increase in population in the towns during th ten years 1861-71.

FIG. V.—Table showing the proportion of persons proceeded against for drunkenness. The numbers for the towns grouped in twenties, year ending Sept. 29, 1876.

FIG. VII. FIG. VI.

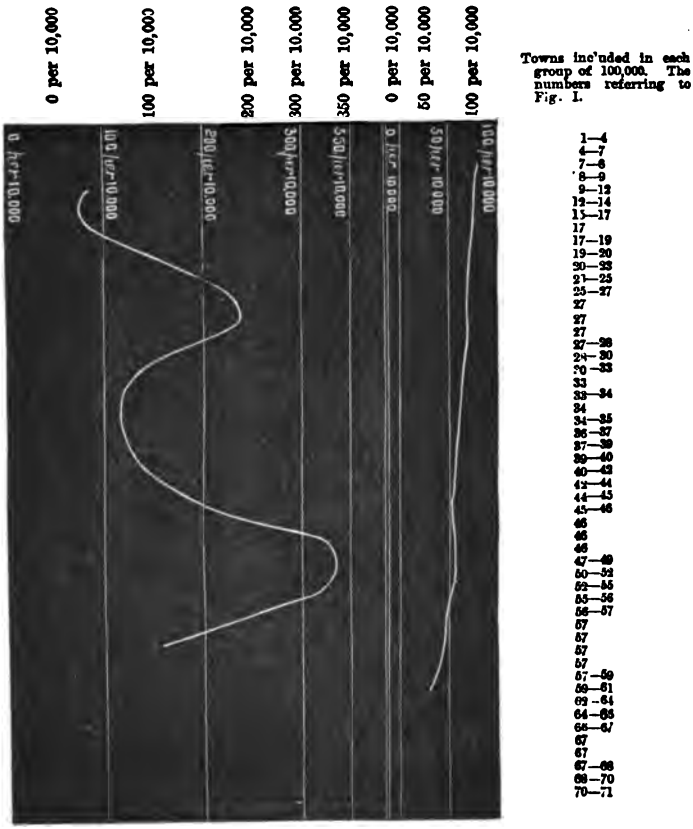

FIG. VI.—Table showing the proportion of public-houses per 10,000 of population. Population grouped in hundred thousands.

FIG. VII.—Table showing the proportion of persons proceeded against per 1,000 of the population. The population grouped in hundred thousands, and these numbers again grouped in tens.

PHYSICAL AND MATHEMATICAL SECTION.

February 27th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

"Results of Rain Gauge Observations made at Eccles, near Manchester, during the year 1876," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of last year was again above the average to a greater extent than that of the previous year. One striking characteristic of the rainfall of last year is the fact that most of the usually driest months of the year were the wettest, and all the wettest months were the driest. The fall of rain for February, March, and April amounted to a trifle over 10¾ inches, whilst the average amount for those months only equals a little over 6⅜ inches; or in other words, these months had six weeks' rainfall in excess of their average. The summer months, viz. June, July, and August, had a trifling excess, whilst the months which are usually the wettest, namely September and October, were amongst the driest. About 8⅓ inches usually falls in those two months, whilst last year the rain that fell in them was only 6¾ inches. The number of days of rainfall was however a trifle below the average. The following table shows the results obtained from a rain-gauge with a 10in. round receiver placed 3ft. above the ground.

Quarterly Periods.		1876.	Fall in Inches.	Average of 16 years.	Differences.	Quarterly Periods.	
Average of 16 years.	1876.					Average of 16 years.	1876.
Days.	Days.						
51	50	January	1·852	2·794	—0·942	7·472	9·512
		February......	3·989	2·221	+1·768		
		March.........	3·671	2·457	+1·214		
46	42	April	3·129	1·989	+1·140	6·749	7·044
		May............	1·084	2·051	—0·967		
		June	2·831	2·709	+0·122		
53	56	July............	3·247	3·224	+0·023	10·685	10·597
		August.........	3·599	3·349	+0·250		
		September ...	3·751	4·112	—0·361		
57	55	October........	2·984	4·227	—1·243	10·556	11·202
		November ...	3·208	3·317	—0·109		
		December ...	5·010	3·012	+1·998		
207	203		38·355	35·462	+2·893		

In the next table are given the results obtained from rain gauges of two different kinds, placed in close proximity in the same plane, and 3ft. from the ground, the one has a 10in. round receiver, and the other a 5in. square one. The difference between the amount that fell in each kind of gauge is very small. It is not always the same amount of difference, nor of the same kind, for the excess or insufficiency is sometimes in the one and sometimes in the other gauge, the average difference for the whole year amounts only to $\frac{18}{1000}$ of an inch. Thus, as I have said before, these gauges are good checks upon each other.

				From 1867 to 1875.		
1876.	Rainfall in inches, in 10in. round receiver 3 feet from the ground.	Rainfall in inches, in 5in. square receiver, 3 feet from the ground.	Difference.	Average of Nine Years' Rainfall in inches, in 10in. round receiver 3 feet from the ground.	Average of Nine Years' Rainfall in inches, in 5in. square receiver, 3 feet from the ground.	Difference.
January ...	1·852	1·851	—·001	2·945	2·998	+·053
February...	3·989	4·019	+·030	2·198	2·170	—·028
March	3·671	3·638	—·033	2·366	2·383	+·017
April	3·129	3·206	+·077	1·994	1·980	—·014
May	1·084	1·062	—·022	1·905	1·876	—·029
June.........	2·831	3·147	+·316	2·579	2·591	+·012
July	3·247	3·015	—·232	3·158	3·128	—·030
August	3·599	3·571	—·028	3·397	3·375	—·022
September.	3·751	3·647	—·104	4·034	3·987	—·047
October	2·984	2·971	—·013	4·761	4·745	—·016
November..	3·208	3·184	—·024	3·265	3·292	+·027
December..	5·010	5·025	+·015	3·343	3·289	—·054
	38·355	38·336	—·019	35·945	35·814	—·131

In the next table I give the results obtained from two exactly similar gauges, placed at different heights from the ground, and free from every interference; each gauge has a 5in. square receiver, and the one is placed 3ft. and the other 34ft. above the ground. The total fall for the year in the one 3ft. from the ground was 38·336 inches, and in the one 34ft. from the ground it was 32·402 inches. The difference between the fall in the two gauges is 5·934 inches, or about 15 per cent less rain fell in the higher gauge than in the lower. There was a similar difference last year with a similar amount of rainfall. In the same table I give the average fall in the same gauges for nine years, and by comparing the results, it will be found that the average difference between the fall in the two gauges is about 17 per cent, very nearly the difference I showed last year on an eight years' average.

1876.	Rainfall in inches in 5in. square receiver, 3ft. from the ground. 1876.	Rainfall in inches in 5in. square receiver, 34ft. from the ground. 1876.	From 1867 to 1876.	
			Average fall of rain in inches for nine years, in 5in. square receiver, 3 feet from the ground.	Average fall of rain in inches for nine years, in 5in. square receiver, 34 feet from the ground.
January	1·851	1·411	2·998	2·127
February ...	4·019	3·231	2·170	1·658
March.........	3·638	2·926	2·383	1·879
April	3·206	2·572	1·980	1·659
May............	1·062	0·987	1·876	1·873
June	3·147	2·470	2·591	2·247
July............	3·015	2·912	3·128	2·829
August	3·571	2·942	3·375	2·862
September...	3·647	3·332	3·987	3·439
October	2·971	2·700	4·745	3·993
November ...	3·184	2·882	3·292	2·628
December ...	5·025	4·037	3·289	2·635
	38·336	32·402	35·814	29·829

The following table gives the ratios of the excesses of rainfall 3ft. from the ground over the amount measured at 34ft. from the ground. As I have before shown, any single year shows no regularity in the succession of these ratios. They are as intermixed for the last year as on any other single year. But when six, or any higher number of years are taken together, and their average ascertained, then a definite order is at once visible. The following table presents a nine years' average of these ratios, and they occur in precisely the same order as they do in a six, seven, and eight years' average. Thus, these averages show that the greatest difference between the amounts of rain which fell in the lower and higher gauges occurs in January, decreases gradually in difference till May or June, and, with the exception of a slight decrease in September, it continues to increase till the maximum is again attained in January. I cannot see that this rule demonstrates anything but what seems to be the fact that more moisture is found in the air to a height 34ft. from the ground, at least, during January than in any other month of the year, and that the least of it is found in May and June. In the same table I give the amount of the mean humidity of the atmosphere, calculated from observations of the dry and wet bulb hygrometer, by means of Glaisher's tables. These results will be found almost entirely identical with the ratios of differences of rainfall, and seem clearly, and at once, to point to the cause of the differences of rainfall between the higher and lower gauges.

1876.	Ratios of such rainfall for 1876.	Ratios of such rainfall for average of nine years, from 1867 to 1876.	Mean humidity of the atmosphere for nine years, from 1867 to 1876.
January	·762	·709	·874
February	·803	·764	·856
March	·804	·788	·840
April	·801	·837	·770
May	·930	·998	·753
June	·784	·867	·751
July	·965	·904	·761
August	·823	·836	·793
September	·914	·862	·778
October	·908	·841	·841
November	·905	·798	·856
December	·803	·801	·872
Annual Ratios	·850	·833	·812

In the next table I give the fall of rain for 1876, during the day, from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. There does not at present seem to be any rule manifest as to the frequency of when the day fall shall exceed the night fall, or the reverse. From 1868 to 1873 the day fall always was greater than the night fall. But, in 1874, the night fall was in excess, and in 1875 the day fall was again in excess, and last year the night fall was again in excess. The excess of last year's night fall over the day was about 6 per cent.

1876.	Rainfall in inches, from 8 a.m. to 8 p.m.	Rainfall in inches, from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·226	0·625	—0·601
February	2·007	2·012	+0·005
March............	1·335	2·303	+0·968
April	1·718	1·488	—0·230
May...............	0·684	0·378	—0 306
June	2·007	1·140	—0·867
July...............	0·772	2·243	+1·471
August	1·371	2·200	+0·829
September......	1·702	1·945	+0·243
October	1·654	1·317	—0·337
November	1·720	1·464	—0·256
December	2·017	3·008	+0·991
	18 213	20·123	+1·910

In the next table I present the average day and night fall for a period of nine years. The results of this table continue, however, to show, though by a very small amount, that the day fall exceeds the night fall, so far as the whole year is concerned. The months which have an excess of rainfall in the night are the same as those shown in the eight years' average, namely—January, February, August, September, and December.

AVERAGE OF NINE YEARS, FROM 1867 TO 1876.

1876.	Rainfall in inches, from 8 a.m. to 8 p.m.	Rainfall in inches, from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·346	1·652	+0·306
February	0·956	1·214	+0·258
March............	1·214	1·169	—0·045
April	1·128	0·852	—0·276
May...............	1·104	0·772	—0·332
June	1 524	1·067	—0·457
July...............	1·697	1·431	—0·266
August	1·624	1·751	+0·127
September......	1·856	2·131	+0·275
October	2·482	2·263	—0·219
November	1·686	1·606	—0·080
December	1·425	1·864	+0·439
	18·042	17·772	—0·270

Annual General Meeting, April 17th, 1877.

R. Angus Smith, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Report of the Council, April, 1877.

The Treasurer's account shows that the ordinary balance is £46 2s. 11½d. against £94 8s. 7d. on the 31st of March, 1876; but there is owing to the Society one year's rent from the Geological Society, and half a year's rent from the Scientific Student's Association, which makes an available balance of £87 10s. against £88 last year.

The number of ordinary members on the roll of the Society on the 1st of April, 1876, was 163 and 7 new members have since been elected; the losses are—deaths 1; resignations 4; defaulters 3. The number on the roll on the 1st instant was 162. The deceased member is Mr. Henry Deacon, F.C.S.

Mr. Henry Deacon was born in London on the 30th July, 1822, and was apprenticed to Messrs. Galloway and Sons, Engineers, in London, at the age of 14, when he left school; his taste at that time having been chiefly in the direction of Mechanics. He obtained however a taste for Chemistry from Faraday, and may be regarded as one of the few pupils of that distinguished man. As Faraday did not formally take pupils, this exception may be considered as a great favour owing to family friendship; and to this part of his life Mr. Deacon often referred with great pleasure. Mr. Deacon left the firm of Galloway and Sons and was engaged at a no less eminent firm, that of Nasmyth and Gaskell of Patricroft, near Manchester, and soon by his ability and industry rose to be held in very friendly relations by those around him.

His mechanical knowledge caused him to be soon removed to become manager at the Plate Glass Works of Messrs. Pilkington at St. Helens, in which a large amount of machinery was used; but as there was some chemical knowledge needed also, and as there was a Laboratory at the works, he was not slow in taking advantage of the opportunity and reviving the love of chemical experiments. This caused another step in his life, and he was made manager in the Alkali Works of Mr. Hutchinson, who began, one may say, in the town of Widnes. At that place he entered into partnership in 1855 with Mr. Gaskell, who was a partner in the firm at Patricroft. The works which they then built have been, and still are, carried on by the firm of Gaskell, Deacon, and Co., one of the largest of the kind.

Mr. Deacon will probably be best known in connexion with the process for making Chlorine by the decomposition of Muriatic Acid acting under heat upon a Salt of Copper. The process is continuous, and was believed to come under the category of those which by Berzelius were called "Catalytic."

It is said that among other circumstances connected with his early life in Faraday's laboratory, Mr. Deacon dwelt with interest upon a question put to him by that philosopher, namely, "How is it that snow disappears when the temperature of the ground and of the air are both below the freezing point?"—and he held that the course of reasoning which this produced led him after many years to the invention of the new process for making Chlorine. In a paper which he read before the Chemical Society he sums up his views in the following manner (*Journal of the Chem. Soc.*, vol. X., 1872):

I. "With the same mixture of Gases and at the same temperature the amount of Hydrochloric Acid decomposed by the aid of a molecule of Copper Salt in a given time depends upon the number of times the molecules of the mixed Gases are passed through the sphere of action of the Copper Salt. Conversely, that the activity of a molecule of Copper Salt depends upon the speed with which fresh matter is presented to it and the products are removed from it."

II. "That in long parallel tubes of the same diameter the number of opportunities of action in the same time is nearly the same at all the velocities of the current of Gas."

III. "That in long parallel tubes of different diameters the number of opportunities of action of each molecule of Copper Salt is the same with the velocities of the current of Gas are in converse proportion to the squares of the tubes' diameters."

IV. "That in porous masses the opportunities of action increase with increased velocities with the current of Gas in nearly direct proportion."

V. "That other conditions remaining the same, the percentage of Hydrochloric Acid decomposed in any given time varies with the square root in proportionate volumes of Oxygen and Hydrochloric Acid."

VI. "That the Cupric Chloride formed bears no definite proportion to the quantity of Chlorine produced."

VII. "That as the sphere of action includes molecules not in contact with the Copper Salt, therefore Hydrochloric Acid must be decomposed under circumstances where the union of either element with the Copper Salt is impossible; that is, that the decomposition must in part, if not entirely, be caused by the result of the forces engaged, and therefore direct from Hydrochloric Acid of Oxygen to water of Chlorine. The amount of chemical change occurring in any given time is not merely a measure of the chemical forces engaged, but is in reality a resultant of all the forces engaged, physical and mechanical as well as chemical."

This idea he calls the parallelogram of forces, which he considers analagous to that in mechanics. By this new process of making Chlorine, Hydrochloric Acid, when freed and dried, is exposed to extended surfaces containing a slight amount of Copper Salt. These surfaces are made of burnt clay or other material. The Chlorine that is formed is passed through a washing apparatus to remove any undecomposed Hydrochloric Acid, and being dried, is conveyed to the bleaching-powder chambers. These chambers are very different from those used in the old places, which were simply large enclosed spaces. Mr. Deacon's chambers are filled with layers of shelves placed one above another: on these the lime is laid, and the Chlorine is passed slowly over them one after another.

Mr. Deacon was a man of great activity, taking an interest not only in chemical manufactories and science, but also in the prosperity and comfort of the inhabitants of the district in which he lived. He was chairman of the first Local Board, and of the first School Board, and his judgment on all points connected with the welfare of Widnes, as well as of the alkali trade, was justly considered worthy of the highest attention and respect. He also attended as a magistrate of the county with great diligence. For several years his health had been very uncertain, but his own belief was that his constitution had surmounted the difficulty with which it was struggling, when he was seized with an attack of typhoid fever, of which he died, after only a week's illness, on the 23rd of July, 1876, at the age of 53.

The late Dr. Dalton's apparatus and instruments lent to the Loan Exhibition at South Kensington have been safely returned, and the Council are taking steps for their permanent preservation and exhibition in the Society's Rooms.

In December last the Council forwarded to the Royal Commissioners of the Great Exhibition of 1851 a memorial praying for a grant of £10,000 out of the surplus funds of the Exhibition to be applied for the benefit of the Society; and in reply they have been informed that the Commissioners are not about to close their trust or distribute their surplus funds.

At a meeting of the Council held on the 6th of March last a resolution was passed unanimously authorising the President to sign a memorial to the City Council in favour of the removal of the Reference Library from Campfield to the Old Town Hall, in King Street.

The following papers and communications have been read at the ordinary and sectional meetings of the society during the present session:—

October 3rd, 1876.—"On the Action of Water and Saline Solutions upon Lead," part II., by M. M. Pattison Muir, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

October 9th, 1876.—"Address on the recent Progress of Natural History," by Professor W. Boyd Dawkins, F.R.S., &c.

October 17th, 1876.—"On the Protection of Buildings from Lightning," by Joseph Baxendell, F.R.A.S.

"On a Remarkable Meteor," by E. W. Binney, F.R.S., &c., President.

"On the Directions of the Face Joints of Oblique Arches," by J. B. Millar, B.A., communicated by Professor O. Reynolds, M.A.

October 31st, 1876.—"On the Arctic Expedition," by Professor O. Reynolds, M.A.

"On a Portrait of Tycho Brahe," by Samuel Crompton, M.D.

"Remarks on the General Affections of the Barometer, noticed by Mr. J. A. Broun," by Professor B. Stewart, LL.D., F.R.S.

"On the Manner in which Raindrops and Hailstones are Formed," by Professor O. Reynolds, M.A.

November 6th, 1876.—"Wealden Fossils from Columbia, South America," by John Plant, F.G.S.

"The Raised Beaches of County Antrim; their Molluscan Fauna, and Flint Implements," by Mark Stirrup, F.G.S.

November 14th, 1876.—"On the Meteor of August 15th," by Mr. John Petrié, communicated by the President.

"On the Eucalyptus," by E. W. Binney, F.R.S., &c., President.

"Notice of a passage in Clement of Alexandria on the Origin of certain Arts and Customs, and their Introduction into Greece," by William E. A. Axon, M.R.S.L., &c.

November 28*th*, 1876.—"The Radiometer," by Harry Grimshaw, F.C.S.

"Note on a Manganese Ore, from New South Wales, and on a Specimen of Native Silver from New Zealand," by M. M. Pattison Muir, F.R.S.E.

December 4*th*, 1876.—"Notice of a Botanical Excursion in the Aberdeenshire Highlands in July, 1876," by Mr. Thomas Rogers.

December 5*th*, 1876.—"Declinations of the Magnet deduced from Observations taken at Eccles," by Thomas Mackereth, F.R.A.S., F.M.S.

December 12*th*, 1876.—"On the Meteor of August 15th," by Mr. N. Staples, communicated by the President.

"The Lowest Amounts of Atmospheric Pressure during the last Sixteen Years, as observed at Eccles," by Thomas Mackereth, F.R.A.S., F.M.S.

"On the use of Anneline Colours in Tinting Photographs," by Joseph Sidebotham, F.R.A.S.

"On a Mineral Water from Humphrey Head, near Grange-over-Sands, North Lancashire," by Joseph Barnes and Harry Grimshaw, F.C.S.

"On Ternary Differential Equations," by Sir James Cockle, F.R.S., corresponding Member of the Society.

December 26*th*, 1876.—"Notice of the 'Almanacke for XII Yere,' printed by Wynkyn de Worde in 1508," by William E. A. Axon, M.R.S.L., &c.

"A Notice of some Organic Remains from the Manx Schists," by E. W. Binney, F.R.S., F.G.S., President.

"On Changes in the Rates of Mortality from different Diseases during the Twenty Years 1854—73," by Joseph Baxendell, F.R.A.S.

January 9*th*, 1877.—"On the early Importations of Cotton into England," by E. W. Binney, F.R.S., &c., President.

"On the Poisonous Properties of Yew-leaves," by James Bottomley, D.Sc.

"On the Luminous Sulphides of M. Ed. Becquerel," by William Thomson, F.R.S.E.

"On the types of Compound Statement involving four Classes," by Professor W. K. Clifford, M.A., F.R.S. Communicated by Professor W. S. Jevons, M.A., F.R.S.

January 15*th*, 1877.—"On the Occurrence of Cratægus Oxyacanthoides (Thuill.) in the neighbourhood of Manchester," by Charles Bailey, Esq.

"List of Desert Plants collected at Ramleh, near Alexandria, Egypt, from September, 1875, to April, 1876," by H. A. Hurst, Esq.

January 23*rd*, 1877.—"Results of the Monthly Observations of the Magnetic Dip, Horizontal Force, and Declination, made at the Magnetic Observatory of the Owens College, from January, 1874, to December, 1876, inclusive," by Professor Thomas H. Core, M.A. Communicated by Professor Balfour Stewart, LL.D., F.R.S.

February 6*th*, 1877.—"Notice of the Junior Literary and Philosophical Society of Manchester, 1806-1807," by William E. A. Axon, M.R.S.L.

"On Compound Combinations," by Professor Cayley, F.R.S., &c.

"On Ternary Differential Equations," by Robert Rawson, Esq., Hon. Member of the Society.

"On various Forms of Vortex Motion," by Professor O. Reynolds, M.A.

"On the powerful Oxidising Action of Animal Charcoal upon Organic Matters as shown by the Analysis of the Drainage from a large heap of a Mixture of Nightsoil and Animal Charcoal," by William Thomson, F.R.S.E.

"A Plea for the Word 'Anglo-Saxon,'" by Rooke Pennington, LL.B., F.G.S.

February 12*th*, 1877.—"Contributions to the Life History of *Acidalia Contiguaria* (Hübner)," by Joseph Sidebotham, F.R.A.S.

"Additions to the List of Shells from Cymmeran Bay, Anglesey, Part IV.," by John Plant, F.G.S.

"Footprint of 'Dinornis,' Bones of 'Dinornis' and 'Dodo,'" by John Plant, F.G.S.

"Upon Specimens from the Fresh Water Deposits of La Limagne d'Auvergne in Central France," by Mark Stirrup, F.G.S.

February 20*th*, 1877.—"Note on a Curious Allusion of a Writer of the 17th Century to a supposed Property of the Magnetic Needle, since verified in the Invention of Telegraphy," by Harry Grimshaw, F.C.S.

"On the Action of Water and Dilute Saline Solutions upon Lead, Part III.," by M. M. Pattison Muir, F.R.S.E.

"On the Increased Mortality from Small-Pox," by Joseph Baxendell, F.R.A.S.

February 27*th*, 1877.—"Results of Rain-Gauge Observations made at Eccles, near Manchester, during the Year 1876," by Thomas Mackereth, F.R.A.S., F.M.S.

March 6*th*, 1877.—"Note on Early Anticipations of a Magnetic Telegraph," by Professor W. S. Jevons, M.A., F.R.S.

"Note on a Passage in Strada containing a Prevision of the Electric Telegraph," by William E. A. Axon, M.R.S.L., F.S.S.

"On the Law of Force when a Thin Homogeneous Spherical Shell exerts no Attraction on a Particle within it," by J. H. Poynting, B.A., B.Sc.

"Enquiries concerning a Change of Position of the Earth's Axis," by Arthur Wm. Waters, F.G.S.

March 20*th*, 1877.—"On the Action of Sea-Water upon Lead and Copper," by William H. Watson, F.C.S. Communicated by Dr. R. Angus Smith, F.R.S.

"Note on the Upper Coal Measures of Canobie, Dumfriesshire," by E. W. Binney, F.R.S., &c., President.

"Losses and Gains in the Death-toll of England and Wales during the last 30 years," by Arthur Ransome, M.A., M.D.

April 3*rd*, 1877.—"On the Action of Water and dilute Saline Solutions upon Lead. Addendum to Part III.," by M. M. Pattison Muir, F.R.S.E.

"The Drunkenness Statistics of the Large Towns in England and Wales," by J. H. Poynting, B.A., B.Sc.

Several of the above papers have already been printed in the new volume of Memoirs, and others have been passed by the Council for printing.

The Council consider it desirable to continue the system of electing Sectional Associates, and a resolution on the subject will be submitted at the annual meeting for the approval of the members.

The Librarian reports that since the last annual meeting a further large number of volumes have been bound; and that the number of societies with which we correspond continues about the same as last year.

THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Balance Sheet from April 1st, 1876, to March 31st, 1877.

Dr.

		£ s. d.	£ s. d.	£ s. d.
1876. April 3.—To	Balance from last Year			236 19 2
,,	*Members' Contributions:*			
	Arrears, 1874-5—2 at 42s.	4 4 0		
	,, 1875-6—12 at 42s.	25 4 0		
			29 8 0	
	164 Members on Roll, at 42s.	344 8 0		
	Less Compounders 3 } ,, in Arrear ......16 } ,,	39 18 0		
			304 10 0	
	6 Admission Fees, at 42s.	12 12 0		
	6 Subscriptions—5 at 42s., 1 at 21s.	11 11 0		
			24 3 0	
	2 Microscopical Section Associates		1 0 0	
	2 Subscriptions in advance, 1877-8		4 4 0	
			363 5 0	
,,	Sale of Publications		5 6 0	
,,	*Sundry Income:*			
	Microscopical Section contribution	2 2 0		
	Physical Section ,,	2 2 0		
			4 4 0	
	Geological Society, use of Rooms, 1875-6	30 0 0		
	Scientific Students	11 10 0		
			41 10 0	
	Bankers' Interest		3 17 1	
				418 2 1
,,	1 Year's Interest on £1,500 Natural History Fund from Owens College			59 6 3
1877. April 1.—	*General Balance:*			
	Compounders' Fund	£98 15 0		
	Natural History Fund, bal. last yr. £43 15 7			
	,, ,, 1 yr.'s Int.. 59 6 3			
		103 1 10		
	Other Balance	46 2 11½		
		£247 19 9½		£714 7 6

Cr.

		£ s. d.	£ s. d.
1877. March 31.—By	*Charges on Property:*		
	Chief Rent	12 15 5	
	Insurance	7 0 0	
	Property Tax	2 2 6	
	Repairs	2 0 3	
			23 18 2
,,	*House Expenditure:*		
	House Tax	6 7 6	
	Coals, Gas, Candles, Water	20 8 0	
	Cleaning and Petty Expenses	5 14 7	
	Tea and Coffee at Meetings	18 1 4½	
	Barometer Repairing and New Case	4 9 0	
			55 0 5½
,,	*Administration Charges:*		
	Wages of Keeper of Rooms	57 4 0	
	Attendance on Sections and Societies	19 4 0	
	Postage and Parcels	38 19 9	
	Stationery and Circulars	15 2 11	
			130 10 8
,,	*Publishing:*		
	Memoirs, Engraving only	4 18 6	
	Proceedings	57 2 6	
	Editor of Publications	50 0 0	
			112 1 0
,,	*Library:*		
	Books and Periodicals	61 9 0	
	Binding	45 4 5	
	Assistance in Library	36 2 0	
	Subscription to Ray Society	1 1 0	
,,	Palæontographic Society	1 1 0	
			144 17 5
	Total Expenditure		466 7 8½
	Balance		247 19 9½
			£714

April 6th, 1877.

Audited and found correct,

A. BROTHERS.
THOS. MACKERETH.

SAMUEL BROUGHTON,
Treasurer.

On the motion of Mr. A. W. WATERS, seconded by Mr. J. A. BENNION, the Report was unanimously adopted, and ordered to be printed in the Society's Proceedings.

On the motion of Mr. R. F. GWYTHER, seconded by Mr. J. H. POYNTING, it was resolved unanimously,

That the system of electing Sectional Associates be continued during the ensuing Session.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:

President.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

Vice-Presidents.

EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.
JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A.

Treasurer.

CHARLES BAILEY.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Of the Council.

REV. WILLIAM GASKELL, M.A.
ROBERT DUKINFIED DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
CARL SCHORLEMMER, F.R.S.
WILLIAM E. A. AXON, M.R.S.L., F.S.S.

PHYSICAL AND MATHEMATICAL SECTION.

April 24th, 1877.

Alfred Brothers, F.R.A.S., in the Chair.

"'Chambers's Journal' and Ozone," by **Thomas Mackereth**, F.R.A.S., F.M.S.

It is a pity that many of our popular journals cannot either let science alone or state correctly and with proper authority what it teaches. I have frequently, from such periodicals, seen the most absurd nonsense set forth as scientific fact. Either the writer of such articles does not understand the subject about which he is writing, or is wilfully disposed, for reasons best known to himself, to pervert the knowledge that has been attained. My attention has been drawn to two numbers of "Chambers's Journal," the one published for June 30th, 1876, and the other for March 31st, 1877. Both these numbers contain articles on ozone, and they are, to say the least of them, the strangest jumble of facts and imagination I ever remember reading.

I would draw attention to the fact that on the 12th of October, 1875, Mr. Baxendell, F.R.A.S., read a paper before this section "On a Source of Atmospheric Ozone," and that on the 29th of February, 1866, Mr. J. B. Dancer, F.R.A.S., read a paper before the section on "An Account of some early Experiments with Ozone, and Remarks upon its Electrical Origin." To do full justice to these gentlemen in what I am about to notice I ought to quote more extensively than I shall from those excellent papers. Mr. Baxendell states, and I quote from his paper first, because it was the first read, that

"In 1872 I had a series of observations made twice daily for five weeks at stations on opposite sides of one of the reservoirs of the Manchester Corporation Water Works, and grouping the results according to the direction of the wind, I found that the mean values for the station on the lee side of the reservoir were not sensibly different from those at the station on the windward side."...... "The results of the observations clearly indicated that mere evaporation from a large continuous surface of water had no effect in increasing the amount of ozone in the air passing over it. Observations and experiments made at Southport, on the action of spray from the sea, led to no satisfactory result, on account of the difficulty of eliminating the effects due to varying velocities of the wind. I had long been anxious to try the effects of spray from a large fountain, but had no opportunity of doing so until June last." Then, after describing the opportunity and the experiments made, he says, "Having now proved experimentally that spray produced by mechanical means, and ordinary fog or haze produced by the condensation of aqueous vapour in the air are precisely similar in their relations to atmospheric ozone, and having also shown that the quantity of ozone usually found in the air could not have been held in solution by the water from which the aqueous vapour in the atmosphere is derived, it becomes evident that the production of atmospheric ozone is in some way dependent upon the minute state of division in which water exists in the air in the visible form of clouds, fogs, and haze, and often also, probably in an invisible form. This conclusion has led me to infer that water exposed to the air has the power of condensing oxygen upon its surface into a thin film of ozone." "Admitting that the inference I have drawn from the facts given in this paper is correct, it will enable us to explain why, on the sea coast, winds from the sea bring more ozone than those from the land."

Here then for the first time is set forth the results of experiments on spray for the production of ozone. Now, I shall show that the writer on ozone in "Chambers's Journal" knew, or if he did not it was his own fault, that Mr. Baxendell made known these results before either of the articles in the Journal appeared. These results were made known and widely circulated in October, 1875; and yet, not till March, 1877, this writer asserts that "It had been noticed that ozone was developed by the spray of water when under pressure." It had been noticed! By whom was it noticed, and when? The article reads as if it had just been noticed by Signor Bellucci, for we are told that he "was thereby induced to make observations at the Falls of Terni to ascertain if ozone was produced by the natural pulverisation of the water, especially as he had often noticed there the characteristic odour of ozone." It seems very strange, if we are to understand that Signor Bellucci "had noticed that ozone was developed by the spray of water," and yet he was thereby induced to make observations to ascertain if ozone was so produced. This is certainly the strangest jumble of words I ever read. Why make observations of what he had noticed, and if he did not notice the fact, who did? Then again we are told, as if Signor Bellucci had originated the idea—"It is noteworthy that the air over the surface of the ocean is richer in ozone than that collected on land." We are left in the dark as to how this has been inferred or ascertained; yet, for further reasons I shall give, there can be but little doubt that the writer of this quotation had the opportunity of learning that in October, 1875, Mr. Baxendell wrote from experimental inference "that water exposed to the air has the power of condensing oxygen upon its surface into a thin film of ozone."

I pass now to still more astonishing statements about ozone in this journal. I have observed that in February, 1876, Mr. Dancer read a paper on ozone. This paper is printed in the Proceedings of the Society. At the head of the paper it is clearly and distinctly stated that the paper was produced and read by Mr. Dancer, and that "E. W. Binney, F.R.S., F.G.S., President of the Section, was in the chair." Notwithstanding this obvious and unmistakable fact, the writer on ozone in the June number of 1876 of the journal, or just four months after Mr. Dancer had produced his paper, states that "We learn from a paper read by Mr. Binney at a recent meeting of the Manchester Literary and Philosophical Society, that the atmosphere of towns may be sensibly ozonised, and, of course improved in quality by the action of public fountains." So that the accuracy of this scientific writer makes Mr. Binney most innocently do duty for Mr. Dancer. But this is not all. In the case of his favourite, Signor Bullucci, he professes to give a quotation from what we thus learn the Signor has written. But Mr. Dancer, our own countryman, is not treated so kindly. Of course we ought to be kind and do the highest justice to the talents of foreigners, but this is no reason why Mr. Dancer, our own countryman, should be plagiarised. I shall not stop to ask what the writer is referring to, when he states that 'A certain sea-side town has been considerably puffed into notoriety as a suitable resort for persons seeking health on account of the quantity of ozone in the atmosphere," he says, "We will not dispute the fact," but if it be a fact, to talk of disputation is sheer nonsense; "but," continues the writer, "it may be doubted whether one sea-side town, more than another, naturally possesses any specially large amount of ozone." If this writer had the knowledge he pretends to have, his doubts on this matter, if facts are anything to him, would soon be removed. Now, I ask, where does the following language come from, which this writer uses as his own? "What, however, is ozone?" "This is a question not easily answered in a satisfactory manner." Mr. Dancer's language, written four months before was identical with this. He writes, "What is ozone? This is a question not easily answered in a satisfactory manner." Mr. Dancer then gives some very good reasons for his assertion. But most ludicrously when the journal asserts "that it is a question more easily asked than answered," proceeds at once, as if all was clear, to answer the question, doubtless, to the writer's satisfaction. He says, too, "still more manifestly is this subtle quality (ozone) produced by the dashing of waves and spray against the air." Doubtless, there is a good deal of truth in this assertion, but who is the author of the idea? Where was it first penned? If the writer copied it, why did he not indicate in the usual way that he had done so? Again, we read in the journal, on its own apparent authority, that "It need only be added, that the delicate and wholesome freshness of the air, after a rattling thunderstorm, is very much due to the development of ozone." How fine all this sounds, especially when it is known that Mr. Dancer had just written in a paper that, doubtless, had been seen by this writer, "That heavy storms of rain, hail, and snow, are always accompanied by free electricity, and a manifestation of ozone. The pleasant sensations experienced on breathing the atmosphere after heavy rain are, perhaps, not altogether due to the washing of the atmosphere, but in part produced by the ozone contained in it." Both Mr. Baxendell's paper and Mr. Dancer's paper were published in the same issue of the "Proceedings" of this Society.

"On a Popular Method of Observing Phenomena on the Surface of the Sun," by J. B. DANCER, F.R.A.S.

The surface of the sun is generally examined by direct vision through the telescope, but observers sometimes project a magnified image of the sun through the eyepiece on to a sheet of white cardboard. This method has the advantage of enabling several persons, at the same time, to view the spots, an eclipse, the transit of a planet, or other phenomena, and when the telescope is mounted in an observatory the extraneous light is easily prevented from interfering with the exhibition.

I have brought for your inspection a contrivance for holding the sheet of cardboard in a convenient manner, with a few additions, which make this method of observing useful to some extent to those who do not possess a micrometer.

The following is a description of the apparatus before you:—The cardboard screen is about six inches square, one side being perfectly plain, and on the other is drawn a circle of about five inches diameter, which is divided by finely-drawn lines into 10 equal spaces both vertically and horizontally. The lower end of the centre vertical line is marked N, the upper end S, and at the end of the centre horizontal line, E and W, in the reverse of their usual positions. This card is made to slide in a groove in a very light wooden frame. A thin brass tube is attached to the lower edge of the frame, and fixed carefully at right angles to the cardboard. The other end of the brass tube has a clip which fits the tube of the astronomical eyepiece. (In this instance I assume that an inverting eyepiece is in use.) The brass tube is bent so that the centre of the clip is exactly opposite the centre of the card screw. On the clip a black cardboard shade of 8 inches square is fixed. This shade prevents the direct light which comes through the opening in the observatory dome from falling on the projected image of the sun. An eyepiece should be selected which will show the entire disc of the sun, and the distance of the cardboard screen so arranged that when the sun's image is sharply defined it exactly fills the circle drawn on the card screen.

To facilitate this, and to make the necessary alterations for the difference in the sun's diameter during winter and summer, there is a sliding adjustment with a clamping-screw. The value of the spaces on the circle can be calculated from the sun's semi-diameter for the day, as given in the "Nautical Almanac."

If the telescope is mounted equatorially, the card circle should be adjusted sideways until a spot on the surface of the sun would keep accurately on the same ruled line crossing the circle. If all these adjustments are carefully performed, the position of a spot at a given time, or the rate of motion of any object crossing the sun's disc, can be approximately obtained with the aid of a good watch. If M. Lescarbault had possessed a simple apparatus of this kind when he observed the transit of the body across the sun in 1859, to which the name of Vulcan has been given, he would probably have saved M. Leverrier and many other astronomers a large amount of valuable time. The members of this Section are doubtless familiar with this method of observation, but there are many in the possession of telescopes who would gladly avail themselves of these suggestions in the absence of the more elaborate method of measurement.

"The Results of Rainfall Observations at Old Trafford, Manchester, for 1875 and 1876," by G. V. Vernon, F.R.A.S., F.M.S.

Rain Gauge 3ft. above the Ground and 106ft. above the Level of the Sea.

Quarterly Periods.		1875.	Fall of Rain.	Average of 32 Years.	Diff.	No. of Days' Rain.	Quarterly Periods.		
1874.	1875.						32 Yrs.	1875.	Diff.
			in.	in.	in.		in.	in.	in.
47	45	Jan.	3·574	2·569	+1·005	21	7·210	5·133	2·077
		Feb.	0·737	2·358	—1·621	10			
		March ...	0·822	2·283	—1·461	14			
40	40	April	0·898	2·016	—1·118	6	7·151	6·527	—0·624
		May	2·069	2·285	—0·216	15			
		June	3·560	2·850	+0·710	19			
55	48	July	4·597	3·562	+1·035	15	10·431	14·330	+3·899
		August ..	4·998	3·538	+1·460	19			
		Sept. ...	4·735	3·331	+1·404	14			
61	37	October .	4·455	3·903	+0·552	14	10·921	9·500	—1·421
		Nov.	4·225	3·783	+0·442	18			
		Dec.	0·820	3·235	—2·415	10			
203	170		35·490	35·713	—0·223	170	05·713	35·490	—0·223

The rainfall during 1875 was 35·490 inches, and fell upon 170 days: the fall was 0·223 inches below the average of the last 82 years.

January had a fall in excess of the average; February, March, April, and May had a fall below the average; June, July, August, September, October, and November all greatly in excess of the average; and December 2·415 inches below the average.

Quarterly Periods.		1876.	Fall of Rain.	Average of 83 Years.	Diff.	No. of Days' Rain.	Quarterly Periods.		
1875.	1876.					1876.	83 Yrs.	1876.	Diff.
			in.	in.	in.		in.	in.	in.
45	55	Jan.	1·744	2·560	—0·716	9	7·237	9·419	+2·182
		Feb.	3·978	2·377	+1·601	24			
		March	3·697	2·300	+1·497	22			
40	37	April	3·156	2·030	+1·126	17	7·140	7·382	+0·242
		May	1·076	2·268	—1·192	6			
		June	3·150	2·842	+0·308	14			
48	53	July	3·570	3·562	+0·008	15	10·424	9·812	—0·612
		August	3·073	3·532	—0·459	17			
		Sept.	3·169	3·330	—0·161	21			
37	53	October	2·601	3·889	—1·288	11	10·916	10·345	—0·571
		Nov.	3·301	3·777	—0·476	17			
		Dec.	4·443	3·250	+1·193	25			
170	198		36·958	35·717	+1·241	198	35·717	36·958	+1·241

The rainfall for 1876 was 36·958 inches, and fell upon 198 days, or 28 more than last year, and was 1·241 inches in excess of the 83 years' average.

The first and second quarters exceeded the average whilst the third and fourth quarters were below the average.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Annual Meeting, May 7th, 1877.

W. BOYD DAWKINS, F.R.S., President of the Section, in the Chair.

The Treasurer's Account and Report of the Council were read and passed.

The Election of Officers for the ensuing year then took place as follows:—

President.
W. BOYD DAWKINS, F.R.S., &c.

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S.
CHARLES BAILEY.
JOHN BARROW.

Treasurer.
H. A. HURST.

Secretary.
J. COSMO MELVILL, F.L.S.

Council.
W. C. WILLIAMSON, F.R.S.
JOSEPH BAXENDELL, F.R.A.S.
A. BROTHERS, F.R.A.S.
JOSEPH SIDEBOTHAM, F.R.A.S.
R. D. DARBISHIRE, B.A., F.G.S.
E. W. NIX, M.A.
THOS. H. BIRLEY (SOMERVILLE).
R. ELLIS CUNLIFFE.

List of Members and Associates of the Section :—

Members:

Alcock, Thomas, M.D.
Bailey, Charles.
Barratt, Walter Edward.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Bickham, Spencer H., Jun.
Binney, E. W., F.R.S., F.G.S.
Birley, Thomas Hornby.
Boyd, John.
Brockbank, W., F.G.S.
Brogden, Henry.
Brothers, Alfred, F.R.A.S.
Cottam, Samuel.
Coward, Edward.
Coward, Thomas.
Cunliffe, Robert Ellis.
Dale, John, F.C.S.
Dancer, John Benj., F.R.A.S.
Darbyshire, R. D., B.A.
Dawkins, W. Boyd, F.R.S.
Deane, W. K.
Heys, William Henry.
Higgin, James, F.C.S.
Hurst, Henry Alexander.
Latham, Arthur George.
Melvill, J. Cosmo, M.A., F.L.S.
Morgan, J. E., M.D.
Nevill, Thomas Henry.
Nix, E. W., M.A.
Piers, Sir Eustace.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S.
Smart, Robert Bath, M.R.C.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist., Owens College.
Wright, William Cort.
Waters, A. W., F.G.S.

Associates.

Hardy, John.
Labrey, B. B.
Linton, James.
Meyer, Adolph.
Peace, Thos. S.
Percival, James.
Plant, John, F.G.S.
Rogers, Thomas.
Roberts, John, M.D.
Ruspini, F. Orde.
Stirrup, Mark, F.G.S.
Tatham, John F. W., M.D.
Warren, Hon. J. B. Leicester
Waterhouse, J. Crewdson.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,

IN ACCOUNT WITH THE TREASURER, H. A. HURST.

Dr.

		£	s.	d.
1876.				
Nov. 28.—	To Reichenbach's Icones Floræ Germanicæ &c. Vols. I.—XXII., 1—14	54	0	0
„	„ Jas. E. Cornish, Periodicals	7	19	1
„	„ Charles Simms & Co., Printing	2	14	0
„ 29.—	„ Cheque Book	0	2	6
Feb. 7.—	„ Smith's Diatomaceæ, 2 vols.	3	0	0
Apl. 9.—	„ Glass Shade for Microscope	0	5	6
„	„ Subscription to L. and P. Society	2	2	0
„	„ Jas. E. Cornish, Periodicals	0	11	8
„	„ Charles Simms & Co., Printing	0	11	0
„ 16.—	„ Zoological Record, Vols. I.—X.	9	9	0
„	„ Subscriptions to Zoological Record Association for 1875, 1876, and 1877	3	0	0
„ 28.—	„ Subscription to "Ibis" for 1877	1	1	0
„	„ C. Simms & Co., Printing Circulars, &c.	2	3	0
„	„ Jas. E. Cornish, Periodicals	1	2	4
„	„ Tea, Coffee, &c.	3	9	4
„	„ Postages	1	2	3
„ 30.—	„ Cash in the Ann-street Branch of the Manchester and Salford Branch	84	16	8
		£177	9	4

Cr.

		£	s.	d.
1876.				
Apl. 26.—	By Balance at Bank	91	8	4
Dec. 31.—	„ Interest allowed by the Bank during 1876	0	16	0
„	„ Subscriptions received from Members and Associates from April 26, 1876, to April 26, 1877	25	10	0
1877.				
Apl. 30.—	„ Grant by Parent Society from Natural History Fund	60	0	0
		£177	9	4

Examined and found correct,

THOMAS H. BIRLEY.

ROBERT E. CUNLIFFE.

24th May, 1877.

[The following paper was received from Mr. Dancer on the 8th of May, 1877.]

"On the Transfer of Subsoil to the Surface by the agency of Worms, Insects, Larvæ, Moles, etc.," by J. B. DANCER F.R.A.S.

Occasionally we hear of the discovery below the surface of the soil of articles which undoubtedly were originally placed on the surface, either by accident or design, and various causes may be assigned to explain the burying process,—such as periodical floods carrying solid matter, sand or dust storms, the formation of fresh layers of mould by the decay of vegetation, and the turning over of the soil during agricultural operations.

From my own observations I incline to the belief that other agencies are concerned in this process. We know that very insignificant operations, if persistently carried on through a lengthened period, will produce important results, and, in this manner, it is possible that earth-boring insects worms, moles, etc., may assist materially in covering the surface with soil transferred from below, and in this way objects may be covered up without the surface of the ground being elevated above its original level, except by the decay of vegetation on its surface.

During the transfer of subsoil to the surface, seeds of plants might accidentally be pushed into conditions favourable for germination, which, in process of time, might change the character of the vegetation on the surface. We know that turning over the ground frequently brings about a complete change in the vegetable growth.

I will name one example which I think, to some extent, supports my views.

Mr. W. Mellor, of the Ardwick Lime Works, caused a small field to be profusely covered with crushed bones; they were not covered over with earth, but left quite exposed on the surface. Some years afterwards, a hole was dug in this field for the purpose of getting some sand from below the soil. On looking into this hole, Mr. Mellor perceived a white band several inches below the surface of the soil, at a uniform depth all round the hole. Curiosity prompted him to examine this white substance, and, to his surprise, he found it to consist of the crushed bones which had been laid on the surface some years previously. In the course of a conversation he asked me if I could account for the altered position of the bones, and, in reply, I said it might possibly have been brought about by the operations of insects and worms, assisted by the mould from decayed vegetation. Mr. Mellor thought this explanation extremely probable, and gave me reasons for his belief.

A brewer in the neighbourhood had a large quantity of unsaleable ale, which he proposed to convert into vinegar, and Mr. Mellor allowed him to place a great number of barrels of this ale in the field. The vinegar making was not, however, successful, and eventually the contents of the barrels were allowed to run on the surface of the field. On the following day, the heaps of worms which lay dead on the ground were so amazing, that if Mr. Mellor had not seen them, he would not have thought it possible for such numbers to have existed in the space.

In caves where large quantities of the bones of animals are found, there is frequently a considerable depth of earth between the layers of bones. Much of this earth may have fallen from the roofs of the caves, and some portion may be the accumulation of dust blown in by the wind; even in these cases the operations of worms and the larvæ of insects may have assisted in the burying process. The animal matter would attract and support a large amount of insect life, worms, etc.

The operations of the lug-worms on the sea shore, during low water, are familiar examples of the transfer of a substratum to the surface. Frequently the holes are of a very considerable depth. If these worms should happen to pass through differently coloured strata, they would leave permanent records of their operations, as the returning tide would fill the holes with the surface material.

Do geologists find any such records in ancient sea-beds?

I have no desire to attach great importance to the agencies named in this short communication, but I think they are deserving of some consideration.

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PROCEEDINGS

OF THE

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

VOL. XVII.

Session 1877-78.

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NOTE.

The object which the Society have in view in publishing their Proceedings is to give an immediate and succinct account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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Report of the Council.—April, 1878, p. 179.

ERRATA.

Page 96, line 3, for $Q_3 = \frac{2}{3}P_1$— read $Q_3 = \frac{P_1}{3}$.

Page 96, line 6, for $2Cu(NO_3)2 + 2N_2O_5$—
read $3Cu(NO_3)_2 + N_2O_5$.

PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 2nd, 1877.

Rev. WILLIAM GASKELL, M.A., in the Chair.

"A Case of Flowering of *Chaemerops Fortunei* (Hook) at Alderley," by ARTHUR W. WATERS, F.G.S.

The fact of Chaemerops Fortunei (Hook) flowering so far north as near Manchester seems to me to be of sufficient interest to be worth mentioning to the Society.

This species, indigenous in Japan, is the most hardy of all the palms. I find that several have flowered in the open air in the neighbourhood of London without any protection; but for it to flower so far north as this is probably very uncommon, though I hear it has done so at York.

The plant in question, in my mother's garden at Alderley Edge, has been there now about 25 years, and has never been covered up in winter. It is about 6ft. high, and has always seemed very healthy; but this summer, for the first time, threw out a racemose male flower. The position of our palm is well sheltered from east and north winds. The garden generally is extremely well protected, so that many of the rarer conifers and other trees and shrubs are scarcely to be surpassed for their fine growth in the neighbourhood of Manchester, thus showing that the palm has grown under specially favourable natural position, and it would be interesting to know if it has or can flower in the neighbourhood in less favoured spots.

The Chaemerops have a special interest from having been found fossil in the Miocene of Europe, and even in very high Arctic latitudes.

Besides the Fortunei, the Ch. excelsa and Ch. humilis are common in England in cultivation. The excelsa is only half hardy, and requires protection under glass in winter. The humilis is a south European species, but flourishes out of doors at Osborne, though it requires protection in the shape of matting in cold weather.

All the Chaemerops grow in temperatures comparatively low when compared with that required by other palms. According to Hooker, the Chaemerops martiana grows at 8,000ft. in the West Himalayas, where they are annually covered with snow.

Some of the Chaemerops Fortunei in Kew Gardens will be about 10 to 12ft., and at the Royal Gardens at Osborne there was in 1860 (Hook) one 10ft. high, which had blossomed for three years running without any protection.

"Table of Effect of Movement of the Surface of the Globe on the Shifting of the Axis of the Earth," by Arthur W. Waters, F.G.S.

After reading my last paper I examined further the influence of the position of land and seas upon the shifting of the axis of the earth, and prepared the following table for a paper elsewhere.

The present distribution was taken, and to find the influence which this would have the globe was divided into 2,500 divisions, viz., 5 degrees of Longitude by 5 degrees of Latitude, and then the effect of movements in one quadrant was calculated by means of Professor Haughton's corrected formula.

The result obtained was that if any water is added to the earth ab extra then it will be so distributed over the ocean that the effect is the same as if $\frac{1}{12}$ of this amount had been added in the Southern hemisphere from the Equator to the South Pole, along a line passing through 45° 44′ East Long. Thus, any movement, such as a submarine elevation which displaces water, would spread it over the oceanic area, and the result, with the present configuration, would be the same as if about one twelfth of the weight had been added in the Southern hemisphere along this line passing through 45° 44′ East, which passes over the Caucasus and through Madagascar.

The following table will show the relative effect of elevations on the position of the pole under different conditions. The two last columns are the most important, as showing the different value of equal elevations in the same Latitude North and South of the Equator in any position near the Longitude mentioned which passes through 45° 44′ East Long.

Elevation of land (already raised above the sea).		Effect of submarine elevation only.	Submarine elevation, together with effect caused by displacement of water.	
LAT.			In the Southern hemisphere, along the line 45° 44′ East (or the opposite Northern hemisphere).	In the Northern hemisphere, along the line 45° 44′ East (or the opposite Southern hemisphere).
0° to 5°	·086	·051	·076	·026
5 — 10	·26	·16	·18	·13
10 — 15	·47	·28	·30	·25
15 — 20	·66	·40	·43	·37
20 — 25	·84	·50	·53	·48
25 — 30	1·00	·60	·62	·57
30 — 35	1·16	·69	·72	·67
35 — 40	1·25	·75	·77	·72
40 — 45	1·28	·77	·79	·74

The geographical position of the land and sea will be always changing, and with it the locality in which the distribution of the water will act. Thus it does not seem at all probable that the shifting caused by movements in one direction will be balanced by those in the opposite direction, and all large movements of the crust consist of minor elevations and subsidences.

For latitudes, 45° to 90°, the tables must be reversed.

As an example of a special case the Caspian area was examined, as it is known that great changes have here taken place in geologically recent times, and that the sea was much more extensive and stood at a higher level. Supposing from this district an amount of water, equal to 200ft. in depth, and of twice the extent of the present Caspian has been removed, this would shift the axis the same as if that weight was added in the Southern hemisphere, and would move the Pole about 166ft. from this cause; but the water thus removed would be spread over the globe so as to move the the Pole, the same as if one-twelfth was added along 45° 44′ E. Long. in the S., thus causing in all a movement of 176ft. (this is supposing the distribution of land and sea remained all the time the same as at present). If there were a continent in the South with such a sea as the Caspian, and similar phenomena took place, then the loss of the water would move the Pole about 166ft. in an opposite direction; but the reapportioning of the water would move it about 10ft. in the same direction as before, so that now, instead of being moved 176ft., the Pole would only be shifted about 156ft.

Ordinary Meeting, October 16th, 1877.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Mr. **Mackereth**, F.R.A.S., stated that he had observed an unusual disturbance of his magnetometer at Eccles on the day preceding the great storm of the 15th, and that similar disturbances often occurred immediately before or during great and violent atmospheric changes.

The **President** exhibited to the meeting some coal measure plants and other organic remains from Spain. He said that Mr. R. C. W. King, of London, had sent him a collection of minerals and fossils to examine. In looking them over he found scales of a *Palæoniscus*, an *Estheria* which his friend Professor T. Rupert Jones, F.R.S., had recognised as *tenella*, and which he (the President) had found in the upper and middle coal measures of Lancashire; *Sigillaria reniformis*, a *Lepidodendron*, and a *Neuropteris*. They were met with in sinking a shaft in search of coal at Puertollano, near Cuidad Real, in the Province of La Mancha.

The following is a section of the strata in descending order:—

No.		Metre. Cent.
1	Vegetable earth	1·35
2	Saponaceous earth	1·35
3	Decomposed slate	1·95
4	Ditto with thin seam of coal	0·05
5	Slate, solid	3·80
6	Thin seam of coal	0·01
7	Slate with fossils	0·20
8	Thin seam of coal	0·02
9	Dark-coloured saponaceous earth	0·20
10	Grey sandstone, hard	2·30
11	Ditto, light coloured	0·31
12	Slate with fossils	1·50
13	Grey sandstone, light coloured	0·02
14	Slate, solid	0·04
15	Grey sandstone, light coloured	0·04
16	Slate, solid	0·04
17	Grey sandstone, light coloured	0·02
18	Slate, solid	1·63
19	Grey sandstone, hard	0·22
20	Slate, solid	1·94
21	Grey sandstone, hard	0·05
22	Decomposed slate with thin coal	0·20
		25·12

From the character of the fossil organic remains and the nature of the strata he was led to believe that the coal field of Puertollano was of the same geological age as our true English coal measures.

Mr. M. M. PATTISON MUIR, F.R.S.E., exhibited and gave a description of a modified form of Hofmann's Apparatus for Determining Vapour Densities.

"Note on an Edible Clay from New Zealand," by M. M. PATTISON MUIR, F.R.S.E.

I lately received from my friend Mr. R. E. Day, M.A., a small specimen of a clay which is greedily eaten by the sheep in a certain district in New Zealand.

The clay was brought by Mr. Day from Simon's Pass Station, Mackenzie country, South Island. It there forms a range of low bare hills: the sheep (merino sheep) eat very considerable quantities of the clay without appearing to be any the worse for it. So far as Mr. Day could learn the clay eating is confined to this particular part of the Islands. It is supposed by the shepherds that the clay must contain salt, and that it is to supply the deficiency of this article of food that the sheep resort to the earth. The analysis shows that very probably the shepherds are right, although one would suppose that to consume so much silica and alumina for the sake of the small proportion of salt was hardly an economical proceeding on the part of the sheep.

The alkali was determined by Lawrence Smith's process of fusion with calcium carbonate and ammonium chloride. The water was determined by heating in a stream of dry air and direct weighing of the moisture expelled: the organic matter, by noting the difference between the amount of water thus obtained and the total loss suffered on ignition.

Silica	= 61·25
Alumina	= 17·97

Ferric Oxide	=	5·72
Calcium ditto	=	1·91
Magnesium ditto...........	=	0·87
Alkalies* (as Chlorides)....	=	3·69
Organic Matter	=	1·77
Water	=	7·31
		100·49

"On the Decomposition of Calcium Sulphate by Alkaline Chlorides. A Contribution to Agricultural Chemistry," by M. M. PATTISON MUIR, F.R.S.E.

In one of his papers upon Diffusion,† Graham describes experiments which prove that a solution of potassium or sodium sulphate in lime water, when allowed to diffuse into lime water, yields a diffusate containing potassium or sodium hydrate: but that a solution of potassium or sodium chloride is not decomposed by lime water under the same circumstances. Graham further shows that when solutions of calcium sulphate and potassium or sodium chloride are mixed no decomposition ensues at the ordinary temperature, but that on boiling the mixed liquids for some time sulphate of sodium (or of potassium) is found, and continues to exist in the cold solution, inasmuch as if lime water be added two or three days after the boiling has been carried out, and the mixture be allowed to diffuse into pure water, caustic soda or potash is found in the diffusate. Graham suggests that if solutions of calcium sulphate and sodium or potassium chloride be allowed to react upon one another for a considerable period of time at ordinary temperatures, a decomposition might slowly take place analagous with that which is quickly produced when the mixed solutions are heated. If such a decomposition indeed take place, we shall have, says Graham, a reaction, which, occurring in the soil, may lead to the ultimate production of those alkaline carbonates required by plants for their nutrition. The steps in the process would be these: — decomposition of the alkaline chloride by calcium sulphate with production of alkaline sulphate; decomposition of the alkaline sulphate by lime, added to or originally present in the soil, with production of alkaline hydrate; transformation of the alkaline hydrate into carbonate by the action of carbonates in the soil, or of carbonic acid in the water and air.

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* The alkali consisted almost entirely of soda. The clay contained small quantities of phosphoric acid.

† On the application of Liquid Diffusion to produce Decomposition: *Chem. Soc. J.*, III. 60.

The investigations of Graham and others have shown that the two last stages are readily accomplished.

2. In order to determine whether the first stage in the series of transformations is or is not attainable, I mixed solutions of calcium sulphate and sodium chloride, and allowed the liquids to remain at the ordinary temperature of the air (15° to 18°) for several weeks. If decomposition had taken place the liquids would contain calcium sulphate, sodium chloride, calcium chloride, and sodium sulphate: the first of these salts is insoluble in ordinary alcohol: hence the addition of alcohol to the mixture would throw down the whole of the calcium sulphate existing as such, without otherwise influencing the state of equilibrium of the various salts in the liquid. In order to determine the amount of calcium sulphate which had undergone decomposition it was therefore only necessary to wash the precipitate produced by adding alcohol, with dilute spirit, and to ignite and weigh it.

The following are my results:

(*a.*) 100 cc. of $CaSO_4$ solution = 191·0 mgms. $CaSO_4$ mixed with 10 cc. of NaCl solution = 233·3 mgms. NaCl.

Amount of $CaSO_4$ in solution after 28 days = 167·0 mgms.

Hence amount of Na_2SO_4 found = 25 mgms.

(*b.*) 100 cc. $CaSO_4$ solution mixed with 50 cc. NaCl solution = 1166·5 mgms. NaCl.

Amount of $CaSO_4$ in solution after 28 days = 103·0 mgrms.

Hence amount of Na_2SO_4 produced = 93·9 mgrms.

(*c.*) 100 cc. $CaSO_4$ solution mixed with 100 cc. NaCl solution = 2333 mgrms. NaCl.

Amount of $CaSO_4$ in solution after 28 days = 64·0 mgrms.

Hence amount of Na_2SO_4 produced = 132·7 mgrms.

3. It is very evident then that Graham's supposition is correct. Without the aid rendered by diffusion time alone is sufficient to bring about a decomposition of calcium sulphate in solution by sodium chloride, also in solution. If this decomposition is then realised in the Laboratory, there can be little doubt that under the more favourable conditions offered by the soil, the decomposition will play a somewhat important part in the nutrition of plant life.

4. The numbers which I have obtained show that the extent of the decomposition under consideration is influenced not only by the time during which the salts are allowed to remain in contact, but also by the mass of sodium chloride employed. The amount of chemical action is almost directly proportional to the mass of the sodium chloride employed, the amount of calcium sulphate remaining constant.

"On some Thionates," by H. **BAKER**, Student in the Owens College. Communicated by Professor C. **SCHORLEMMER**, F.R.S.

Having been lately working on these salts, I offer the following observations:—

Barium Dithionate. $BaS_2O_6 2Aq$. According to Heeren* this salt is soluble in 1·1 parts of boiling water; but he does not give the boiling point of the saturated solution; this I have observed to be 102°, and its solubility at this temperature to be 1 in 0·994 parts of water. It is remarkable that the saturated solution of such a soluble salt should boil at so low a temperature. The sp. gr. of the crystals of this salt at 13°·5 is 4·536.

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*Pogg. Ann. 7, 172.

Lead Dithionate. PbS_2O_64Aq. "Is very easily soluble in water," Watts. I find its solubility at 20°·5 to be 1 in 0·869 parts of water. Its sp. gr. at 11° is 3·259.

Calcium Dithionate. CaS_2O_64Aq. The crystals have a sp. gr. of 2·176 at 11°.

Nickel Dithionate. NiS_2O_66Aq. One part of this salt dissolves in 0·897 parts of water at 12°.

Magnesium Dithionate. MgS_2O_66Aq. According to Watts' Dictionary "forms six-sided tables, very soluble in water;" but Gmelin says it forms ill-defined six-sided prisms.

I obtained it in oblique prisms; and found its solubility at 17° to be 1 in 0·692 parts of water.

Sodium Dithionate. $Na_2S_2O_62Aq$. Its sp. gr. at 11° is 2·175. Watts says, "it crystallizes by spontaneous evaporation in large transparent right-rhombic prisms." Gmelin gives several measurements, but they are not sufficient to calculate all the forms from. My measurements show the crystals to be rhombic, and the axes to be $\breve{a} : \bar{b} : c = 0·9922 : 1·0000 : 0·5981$; the forms occurring are ∞P, $\bar{P}\infty$, P, $\bar{P}\frac{1}{2}$, $\infty\bar{P}\infty$, and the type is long prismatic, through predominance of ∞P. In these and the following angular measurements the interfacial angles are denoted as in "Kopp's Krystallographie," viz.:—

A = the angle of a pyramid, over a vertical edge in the brachy-diagonal.

B = the angle of a pyramid over a vertical edge in the macro-diagonal.

C = the angle of a pyramid over a lateral edge.

W = the acute angle in the vertical prism, and the lateral angle in a dome.

Form.	Angle.	Calculated.	Found.	Gmelin.
P	A	125°45′	125°40′	125°48′
	B	125°18′	125°29′	125°18′
	C	81°11′	———	80°18′
$\bar{P}\frac{1}{2}$	A	88°39′	88°34′	———
	B	137°43′	137°40′	———
∞P	W	89°33′	89°25′	89°22′ & 89°36′
$\bar{P}\infty$	W	62° 9′	62° 3′	62°12′

Silver Dithionate—$Ag_2S_2O_6 2Aq$.—forms, according to Watts, right-rhombic prisms, isomorphous with sodium dithionate. Gmelin gives some measurements. I find the axes to be $\breve{a}:\bar{b}:c = 0{\cdot}9884:1{\cdot}0:0{\cdot}5811$, and the forms $\bar{P}\infty$, P, ∞P, $\bar{P}\frac{1}{2}$, $\infty\bar{P}\infty$, $\infty\breve{P}\infty$; the type is very short prismatic.

Form.	Angle.	Calculated.	Found.	Gmelin.
P	A	126°47′	126°51′	127°
	B	126° 6′	125°55′	126°
	C	79° 9′	79° 8′	79°10′
$\bar{P}\frac{1}{2}$	A	89°54′	89°58′	90°12′
	C	104°58′	104°52′	———
∞P	W	89°20′	89°22′	89° 8′
$\bar{P}\infty$	W	60°54′	61° 1′	———

Silver Sodium Dithionate—$(AgNa)_2S_2O_6 2Aq$—is, according to Watts, "apparently isomorphous with the component salts." I find it to be rhombic with the axes $\breve{a}:\bar{b}:c = 0{\cdot}9813:1{\cdot}0000:0{\cdot}5856$, and the forms $\bar{P}\infty$, P, $\infty\breve{P}\infty$, ∞P, $\infty\bar{P}\infty$; the dome is developed so much as to make the crystals horizontally prismatic, the prism is only slightly developed, and the pyramid $\bar{P}\frac{1}{2}$ does not occur. Watts also says, it "exhibits very distinct cleavage;" this cleavage plane I find to be ∞P, and I also find that the sodium and the silver dithionates exhibit very distinctly this cleavage.

Form.	Angle.	Calculated.	Found.
P	B	125°30′	125°23′
	C	79°48′	79°46′
∞P	W	88°55′	89° 1′
$\bar{P}\infty$	W	61°39′	61°41′

This sample contains % Ag. 32.0
$AgNaS_2O_6 2Aq$ requires % Ag. 33.00

Potassium Trithionate. $K_2S_3O_6$. The following are the statements regarding the crystalline form of this salt:—*Gmelin* gives two (1) "Slender four-sided obliquely truncated prisms (Plessy T. für Pract. Chem. 33, 348)," (2) "Right rhombic prisms with dihedral summits resting on the acute lateral edges (Provostaye N. Ann. Chim. Phys. 3, 354)." According to *Watts* it forms four-sided prisms, bevelled with two faces, and according to *Miller* it crystallizes in the rhombic system, prismatic type with dihedral summits. I obtained it in rhombic needles of ∞P, with the acute edges modified by another prism $\infty \breve{P}2$; $\infty \bar{P}\infty$, and $\infty \breve{P}\infty$ are also present; they are terminated by a brachy dome. Some of the crystals taper like a sewing needle, I suppose through the presence of a very acute pyramid. I find the axes to be $\breve{a} : \bar{b} : c = 0{\cdot}3586 : 1{\cdot}0000 : 0{\cdot}4204$, and the observed forms are ∞P, $\infty \breve{P}2$, $\infty \bar{P}\infty$, $\infty \breve{P}\infty$, $\breve{P}\infty$.

Form.	Angle.	Found.	Calculated.
∞P	W	39°31′	39°28′
$\infty \breve{P}2$	W	71°12′	71°18′
$\breve{P}\infty$	W	45°37′	45°37′

This salt is prepared by acting on a saturated solution of Potassium thiosulphate with SO_2. Now, as it is not stated that sodium thiosulphate will not similarly yield sodium trithionate, nor any reason given why it should not, I attempted to prepare sodium trithionate in this manner, but only obtained crystals of sodium thiosulphate. Rathke (Journ. pr. Chem. 95, 13) prepares sodium trithionate as a white powder by decomposing Potassium trithionate with sodium tartarate.

Ordinary Meeting, October 30th, 1877.

Dr. R. ANGUS SMITH, F.R.S., Vice-President, in the Chair.

Professor OSBORNE REYNOLDS, F.R.S., showed and explained some models illustrative of the manner in which waves enter upon the undisturbed surface of water.

Professor W. BOYD DAWKINS, F.R.S., exhibited and explained the results of the Creswell Caves Exploration of 1876.

Ordinary Meeting, November 13th, 1877.

E. W. BINNEY, F.R.S., F.G.S, President, in the Chair.

"Note on Envelopes and Singular Solutions," by Sir JAMES COCKLE, F.R.S., Corresponding Member of the Society.

1. Leibnitz (1694) showed how to find the curve formed by the continuous intersection of an infinity of curves included, on making the parameter vary, in one and the same equation. This gives rise to a new equation by which a value of the parameter is expressed as a function of the coordinates. And this value substituted in the proposed equation gives at once a finite equation for the required curve. Applying this method to the question of finding the curve whereof the normals have a given relation to the portions of the axis of abscissæ intercepted between the origin and the normals, Leibnitz considers this curve as formed by the continuous intersection of an infinity of circles having their centres on the axis. Then the radii of

the circles become the normals to the curve, and the given relation is supposed to exist between the radii and the abscissæ which correspond to the centres of the circles. Taking the relation to be this, viz., that the square of the normal is proportional to the abscissa of the centre, he found the curve of intersection and solved the problem (Lagrange, *Leçons* 1806, pp. 263, 264). But Leibnitz did not remark that his solution does not admit of an arbitrary constant, while it is evident that the problem leads naturally to a differential equation, whereof the integral cannot be completed without the introduction of an arbitrary constant (*ib.* 265). Nor did John Bernoulli, discussing the same problem in his own way, remark that it belongs essentially to the inverse method of tangents, and that, consequently, the general solution depends on an integration which ought necessarily to introduce an arbitrary constant (*ib.* 272). Neither Leibnitz or John Bernoulli seem to have noticed the sort of contradiction which their solutions, arrived at by different methods, offered to the very principles of the differential calculus (*ib.* 274, 275).

2. Taylor (1716) is perhaps the first who deduced a singular solution directly from the derived equation, and who recognised the singularity. He obtained it by differentiating the differential equation (*ib.* 276). Clairaut (1739) was led to a differential equation whereof he obtained two different solutions by means of differentiation (*ib.* 278). He noticed the singularity, and states generally that there are differential equations which have two different solutions, whereof one may be obtained without using the integral calculus (*ib.* 279). Lagrange showed that, in general, every derived equation is susceptible of a form such that the derivee of this form has two factors, whereof one corresponds to the complete primitive, and the other gives at once the singular solution, if there be one (*ib.* 211, 214, 216, et seq., and compare 180.)

3. Euler (Mechanics) found different examples of these duplex solutions, and even gave rules for discovering them in certain cases. Some years after (in his Memoir of 1756) he expressly occupied himself with the subject (*ib.* 282). He was content to exhibit this duplicity of solutions as a paradox; inasmuch as the equation which contains an arbitrary constant, and which we ought therefore to regard as the complete integral, does not include the other finite equation. This last is, however, equally a solution of the differential equation, contrary, as it seemed, to the principles of the calculus (*ib.* 287).

4. It remained to discover the connection between these singular solutions and the complete integrals, as well as between the curves represented by the one and the other, and to refer the whole theory of these different solutions to the first principles of the calculus. This was done by Lagrange (*ib.* 290; 178).

5. It is the characteristic of singular solutions that they appertain to the curves formed by the continuous intersection of the curves represented by the complete primitive, when the arbitrary constant, a particular value of which distinguishes a particular primitive, is made to vary continuously (*ib.* 268, 269).

BRISBANE, QUEENSLAND, AUSTRALIA,
SEPTEMBER 7, 1877.

On the Formation of Hailstones, Raindrops, and Snowflakes, by Prof. O. REYNOLDS, F.R.S.

My present communication forms a continuation of the paper I read before this Society on the 31st of October, 1876, "On the manner in which Raindrops and Hailstones are formed."

To the contents of this paper I shall have to refer continually—hence in order to render what I have to say intelligible it may be well for me to recapitulate some of the leading points in my former paper. The chief purpose of the paper was to explain the manner in which the minute cloud particles aggregated so as to form raindrops and hailstones.

Aggregation resulting from the more rapid descent of the larger particles.

I commenced by pointing out that as the suspended particles of water or ice which constitute a cloud are all descending with velocities which increase with their size, the larger particles will descend faster than the others, and will consequently overtake those immediately beneath them; with these they will combine so as to form still larger particles which will move with greater velocity and more quickly overtaking the particles in front of them, will add to their size at an increasing rate—and I then proceeded to consider how far this was a sufficient, as well as a necessary, cause of the phenomena of hail and rain. One of the most important points on which my arguments were based was

The Shape and Structure of ordinary Hailstones.

On close observation I had found what had previously been noticed by other observers, that the shape of an ordinary hailstone is not what at first sight it appears to be. They are not spheres more or less imperfect, but more or less imperfect cones or pyramids with rounded bases like the sections of spheres; the conical surface being striated, the striæ radiating from the vertex of the cone.

In texture the hailstones have the appearance of being an aggregation of minute particles of ice fitting closely together but without any crystallization, such as that seen in the snow-flake, while, on careful observation, it is seen that they are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have been broken off in their descent.

As I explained, it seemed to me that this form and structure was exactly such as would result from the manner of aggregation, which I had discovered when a particle which ultimately forms the vertex of the cone starts on its downward career and encounters other particles; these adhere to its lower face. The mass, therefore, grows in thickness downwards, and as some of the particles strike the face so close to the edge that they overhang, the lower face continually grows broader, and a conical form is given to the mass above.

When a particle first starts it moves slowly, and the force with which it meets the other particles is slight, and consequently its texture is loose, but as it increases in size and velocity it strikes the particles which it overtakes with greater force and so drives them into a more compact mass.

Assuming that the temperature at which hailstones are formed is not greatly below 32° the particles must actually freeze together. For the effect of squeezing two pieces of ice together at or near the temperature of 32° is to cause them to thaw at those points where the pressure is greatest, at which points they freeze again as soon as the pressure is removed.

In illustration of the force with which the particles strike the face of the hailstone, I instanced the action of the particles of sand in Mr. Tilghman's sand blast used for cutting glass and other hard materials.

I also reverted to the possibility of making

Artificial Hailstones,

by blowing a stream of frozen fog against a small object, making as it were the cloud to rise up and meet the stone instead of the stone falling through the cloud.

I had not, however, then overcome the difficulty of obtaining such a stream of frozen fog, but I gave two sketches of plaster stones, which as far as their shape and the striated appearance of their surface were concerned, closely resembled hailstones, and which plaster stones had been obtained by blowing some finely divided plaster of Paris against small splinters of wood by means of a jet of steam.

In the discussion which followed my paper, Dr. Crompton suggested

The Ether Spray,

such as is used in surgery as a means of obtaining a frozen fog. And shortly after the meeting I tried this ether spray, using an instrument such as surgeons use. But although I found that the spray would freeze anything such as a small tube of water, I could get no deposit of ice particles on the outside of any object. I varied the form of the apparatus, but with no better success, and for the time I abandoned the attempt.

What the cause of this failure was I do not precisely know, but I attribute it to some excess of alcohol in the ether then used, which was not methylated ether. That this might have been the cause occurred to me about two months ago. I then determined to try again, and combine a spray of water with that of ether. I now obtained the lightest ether which Messrs. Mottershead & Co. could supply. The specific gravity of this was ·717, and it was made from methylated spirit.

With this, somewhat to my surprise, I at once obtained a deposit of ice even without the water spray, and with the same apparatus I had previously used. It was not, however, until I used the combined spray of water and ether that I obtained anything resembling a hailstone in appearance. But the first time I used this combination I obtained a small but well-shaped hailstone on the end of a match, which I held pointed towards the spray.

The next time I tried, however, on another day, I did not succeed so well with the water as without it.

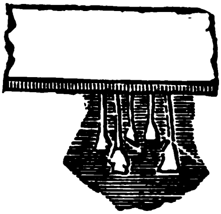
When using the water spray the deposit of ice was wet or half melted, while without the water I obtained a hailstone in much the same manner as I had obtained before with the water.

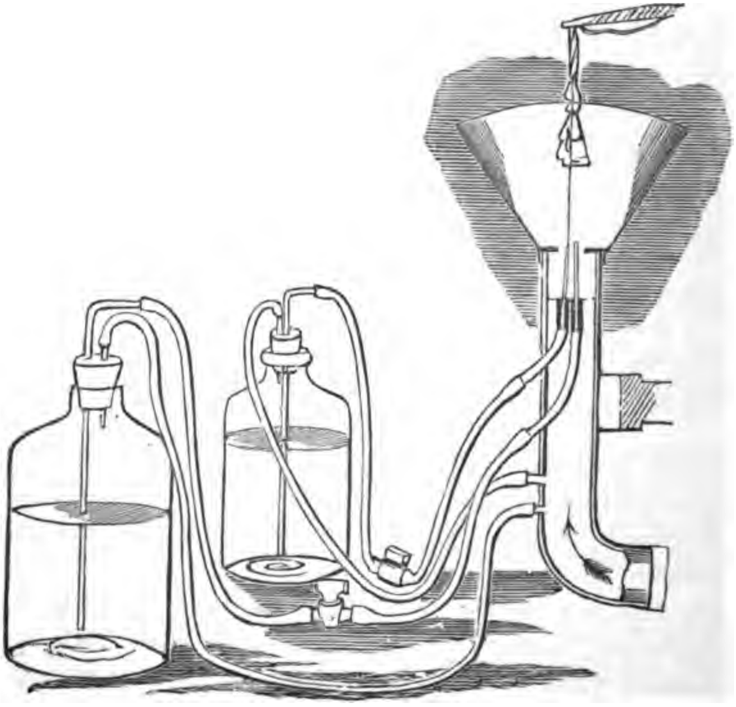
This difference in the results on the two occasions was at once explained by the different states of the air, for on the first occasion it had been cold and dry, whereas on the second it was warm and saturated. With the dry air the ether spray reduced the temperature so far below 32° that the particles of ice did not freeze together—the force of impact was not sufficient to cause them to thaw in the first instance, and hence the water spray was necessary to keep this temperature from falling too low—whereas with the warm saturated air the ether did not reduce the temperature of the air and the vapour it contained much below 32°, and consequently when the water spray was added the water was only partially frozen.

I subsequently improved the apparatus so as to be able to regulate the supply of water and ether to the condition of the air.

The Apparatus.

This is shown in the accompanying sketch. It consists of a brass tube half an inch in diameter, one end of which is connected with bellows capable of maintaining a constant pressure of about 18 inches of water, on the other end of the tube is a cap over the end of which is a flat plate or diaphragm having a central opening ⅛ of an inch in diameter which forms the aperture for the blast. Entering through the sides of the main brass tube are two small brass tubes which reach to within ½ an inch of the plate and into the ends of which are sealed fine glass capillary tubes, the glass being very thin; these protrude just through the middle of the aperture, the one about $\frac{1}{16}$ of an inch and the other $\frac{1}{32}$. Through these tubes the water and ether are separately introduced into the blast to form the spray, and it is mainly on the adjustment of these tubes that the efficiency of the apparatus depends. It is essential that





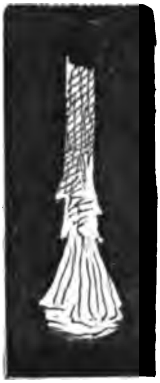
the ether tube should be slightly the longest, otherwise the ends become stopped with ice, and I find it better that the ether tube should be somewhat larger than the water tube. The bore of the tubes must be very small, but this is not sufficient, for unless the glass is very thin the spray will not be finely divided. Both the ether and water are forced through the tubes from bottles by connecting the interiors of these bottles with the bellows, and the quantities of ether and water are regulated either by raising and lowering the bottles or by means of the cocks in the pipes.

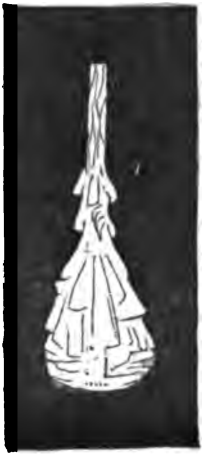
The tube is fixed in an ordinary retort-stand, so that the blast is vertical. If then a small splinter of wood is held downwards pointing into the spray, a lump of ice forms on the end of the splinter, and this lump has all the appearance of the hailstones. It is quite white and opaque, it is conical in form and has a rounded base and striated surface.

In this way I have formed stones from half to three-quarters of an inch in diameter. When, however, the stones are growing large it is neccessary to move this splinter so as to expose in succession all parts of the face of the stone to the more direct action of the spray.

When using this apparatus in a warm room I have found it best to fix a pad of blotting paper over the jet at a height of 10 or 12 inches. The surface of this pad is cooled by the spray and prevents radiation from the ceiling, which otherwise tends to melt the top of the stone. For a similar reason I have found it well to surround the blast with a wide clylinder or inverted cone of paper, which keeps off radiation without interfering with the action of the jet.

By sticking several splinters of wood pointing downwards into the pad, a number of stones may be made at once.





In the accompanying sketch a medium sized stone as well as one of the largest stones are shown attached to the splinters of wood. The surface of the cone, where continuous, is truly conical or rather pyramidal, but this surface is broken, as it were by steps, and a very marked fact is that all the continuous surfaces have the same vertex, and hence the different conical surfaces to which they belong, have not the same vertical angle, the surface being exactly such as would be acquired by the fragments of a sphere so constituted that the fracture tended to follow radial lines.

Owing to the radiation of the surfaces from a common vertex and the steps which occur between the vertex and the base, the angle of the conical surface of the stone is greater near the vertex than near the base. Thus the smaller stones appear less elongated than those which are larger.

The fact that in the sketches of actual stones, which I gave in my last paper, I showed the steps as less pronounced and the angles larger than they are in the artificial stones, is probably owing in some measure to my having formed my ideas from the observation of favourable specimens chosen from amongst those which fell. The larger angles were probably also, in part, owing to the smaller size of the actual hailstones, which were not much more than one fourth of an inch across. But I think that it is important to notice that the somewhat imperfect way in which the outside layers in the surface of the artificial stones are continued, may be owing to the narrowness of the jet of air which, on striking the stone, tends to diverge laterally rather than to flow upwards past the sides of the stone, as it would do if the jet were broader, or as the air must do when the stone is falling through it.

The rate at which stones can be formed depends on the amount of water which can be introduced into the spray, the larger stones taking from one to two minutes. At first sight this may seem to be somewhat slow, but the following estimate tends to show that the artificial are probably formed quicker than the actual stones.

The speed of the jet of air at the point at which the stones are formed is nearly equal to that at which the larger stones would fall through the air. This is shown by the fact that if a large stone becomes accidentally detached from its splinter of wood it rather falls than rises, but when this happens with smaller stones they are driven up by the force of the blast.

I find that the speed of the blast varies from 150 to 200 feet per second, *i.e.*, from one to two miles a minute. The larger stones, therefore, traverse from one to three miles of frozen spray. So that if we imagine a cloud as dense as the spray, it would have to be from one to three miles thick in order that the stones might, in falling through it, attain the size of the artificial stones, and considering that the stones would only gradually acquire a speed equal to that of the blast, the time occupied in falling through the cloud would in all probability be very considerable, at least from five to ten minutes after the stone had acquired a sensible size.

As regards the proportion which the density of spray bears to that of a cloud, a comparison may be made from the fact that when working in saturated air at a temperature of 60° or 70° F., the condensation of vapour supplied sufficient ice to form the spray; and since it is probable that the dense summer clouds, from which hail is formed, result from the cooling of air from temperatures nearly if not quite equal to this, there is probably no great difference in the density of the clouds and the spray. I shall now endeavour to put the apparatus in operation, and exhibit the production of some of these artificial stones.

Snow Crystals.

I have not yet had an opportunity of examining the texture of these artificial stones under the microscope, but to all appearance they consist of an aggregation of small spherical particles of ice; and it seems worthy of notice, that while nothing like a grown crystal appears ever to be produced in the ether spray, the moment the blast is stopped the end of the ether tube becomes covered with ice, which often assumes the form of snow crystals.

This appears to indicate the character of the difference between those conditions which result in snow and those which result in hail.

When the cloud-particles are formed at or above the temperature of 32°, and then freezes, owing to cooling by expansion or otherwise, the particles as they freeze retain their spherical form. This is what happened in the spray.

On the other hand, when saturated air at a temperature below 32° is still further cooled, the deposition of the vapour will be upon ice, and will take the form of snow crystals.

The aggregation of the snow crystals into flakes is, as I pointed out in my previous paper, accounted for by the larger crystals overtaking the smaller crystals in their descent, and the still more rapid descent of the flakes as they increase in size.

As regards the formation of rain-drops, I have nothing to add to what was contained in my last paper. The same explanation obviously applies to both hail and rain; and any doubt which may have been left by the less direct arguments in my former paper will, I venture to think, have been removed by the verification of my predictions in the production of artificial hailstones so closely resembling in all particulars those formed by nature. And, in conclusion, I would thank Dr. Crompton for the suggestion of the means by which I have been able to produce these stones.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 8th, 1877.

Prof. BOYD DAWKINS, F.R.S., President of the Section, in the Chair.

The President gave an address in which he brought before the notice of the Section the three discoveries in Palæontology which stand out beyond the rest in the record of the advance of knowledge in the last session. The evidence offered by the Creswell caves as to the progress of man in the palæolithic age, the full proof by Prof. Marsh of the intermediate position of the cretaceous birds between the reptiles on the one hand and living struthious birds on the other, and the filling in of the gap in the pedigree of the European horse by Dr. Forsyth Major's discovery of *equus stenorus* in the pleiocene of Italy.

He also touched on the problem of evolution, and held that the argument against that doctrine based on the persistence of simple forms such as the invertebrates generally was worthless, because simple structure implies simple needs and simple environment. The change has ever been simplest in the most highly organised. The argument founded on our inability to tell the how or the why also is worthless, because we know the how or why of scarcely anything around us. And lastly that based on the suddenness of the appearance of new forms, as for example the Tertiary flora and Tertiary mammalia was equally worthless, because it depended on the perfection of the geological record and on the absurd idea that we are now in possession of every part of the record. How imperfect the record is may be gathered from the fact that since the Meiocene age the mountains of the west of Scotland have lost ½ of their height. The general conclusion at which he arrived was that evolution is a good working hypothesis, apart from the consideration of the how and the why, and that it is sufficient to prove that certain forms have been evolved from others unlike them, such as the horse from the hipparion, without being able to do it in all cases. In conclusion, the attention of the Section was called to the very unsatisfactory condition of museums in this country, and to the equally unsatisfactory position of natural history in the general scheme of education. The President looked forward to the time when there will be a museum in Manchester which will be a centre around which all the Mechanics' Institutions and natural history societies in the district will group themselves. To hasten this it will be necessary for every individual to use his efforts to influence those immediately around him.

Mr. HURST exhibited a collection of Mediterranean marine algæ; principally from the island of Rhodes, and from the neighbourhood of Alexandria: and a large specimen of an Egyptian cotton plant, from the Ashmouni districts.

Mr. WATERS, F.G.S., exhibited some microscopical sections of Bryozoa, and explained their anatomy: especially dwelling upon the genus Flustra, or horn wracks.

General Meeting, November 27th, 1877.

Dr. R. **Angus Smith**, F.R.S., Vice-President, in the Chair.

Mr. Wilfred Becker, B.A., of Withington Road, Whalley Range, and Mr. Samuel Moore, of Bamford, Derbyshire, were elected Ordinary Members of the Society.

Ordinary Meeting, November 27th, 1877.

Dr. R. **Angus Smith**, F.R.S., Vice-President, in the Chair.

Mr. G. S. **Woolley** presented to the Society a photographic portrait of Dr. Dalton, enlarged from a Daguerreotype taken by the late Mr. John Parry about the year 1841; and also a photograph of the first prospectus of Dalton's School.

On the motion of Mr. **Bailey**, seconded by the Rev. **Wm. Gaskell**, the thanks of the Society were unanimously voted to Mr. Woolley for his valuable donations.

"On the Origin of some Ores of Copper" (Part I.), by **Charles A. Burghardt**, Ph.D., The Owens College.

To endeavour to trace the various ores of copper to one fundamental source, and to study the relationships probably existing between the copper compound and its accompanying minerals, has appeared to me a subject of great interest, in fact, one which has engaged my attention for some time. The commonest occurring ore of copper is undoubtedly cuprite (Cu_2O) either independently, or more often associated with malachite, azurite, brown iron ore, and native copper. Scarcely a specimen of copper ore can be obtained (with the exception of the sulphides) which does not exhibit more or less cuprite in intimate intermixture with the other ore; whence it must be inferred that all, or almost all the ores of copper have been formed by aqueous and not by plutonic action, as cuprous oxide is undoubtedly prepared with greater facility by the wet method (that is from cuprous or cupric solutions) at a comparatively low temperature than by the dry method at a very high temperature. I will briefly state in which way cuprite (that is the crystallised cuprous oxide) has been obtained by different authors, firstly by the dry method and secondly by the wet method.

PREPARATION OF CUPRITE.

Dry Methods of Preparation of Cuprite.

1. A mixture of 24 parts of cupric sulphate and 29 parts of copper turnings heated to redness in a well closed crucible furnishes eventually a reddish-brown crystalline mass which is cuprite.

2. A mixture of 100 parts of cupric sulphate and 57 parts of soda crystals is heated until all the water of crystallisation is driven out, the residue powdered finely, and 37 parts of copper turnings are added, and the whole mixture heated in a crucible to a white heat for half an hour. On washing out the cooled mass with water, cuprite of a very fine colour is obtained. (Wöhler and Liebig, Pogg. Ann. XXI. 581.)

3. Crystals of this substance are often observed in the slags obtained in the smelting of copper ores.

It will readily be observed from the above that an intense heat is requisite for the formation of cuprite by the dry method.

Wet Methods of Preparation of Cuprite.

1. A solution of equal parts of cupric sulphate and grape sugar is mixed with sufficient caustic soda solution to dissolve all the resulting precipitate and then gently heated, cuprous oxide then separates out in the form of a crystalline powder. (Mitscherlich Journal Pract. Chem. XIX. 430.)

2. Bequerel (Compt. Rend. XLIV. 308) describes a beautiful method which is applicable for the preparation of many minerals. He filled a test tube with a neutral solution of cupric nitrate, placing a little cupric oxide and a clean strip of copper plate at the bottom, closed the tube airtight and left it to itself for many months. In this way he succeeded in obtaining small shining cubes of cuprite.

3. A. Knop (Jahrb. f. Min. 1861, 508) states that if a mixture of solutions of cupric sulphate and ferrous sulphate be treated by an alkaline carbonate, carbonic acid gas is evolved and a mixture of ferric hydrate and hydrated cuprous oxide is precipitated, the latter becoming crystalline after the expiration of some time.

Of the three methods given above that of Knop undoubtedly furnishes one of the most probable processes by which many large deposits of the so-called tile ore and copper pitchblende have arisen; but there are again many other deposits of cuprite which may easily have been formed in other ways. I will not discuss at the present time the various opinions upon the formation of copper ores expressed by some very eminent and competent authorities, but I may mention that Dr. Ferdinand Wiebel, in a lengthy and exhaustive treatise (Das Gediegen Kupfer und Roth Kupfererz, Hamburg, 1864) concludes that the native metal and cuprite have in *all* cases been formed from a cupric sulphate solution (obtained by the oxidation of copper pyrites) through its reduction by a solution of ferrous sulphate (also derived from the oxidation of copper pyrites). This statement is doubtless correct in the case of some formations, but there are many others where it is questionable. Gustav Bischoff points out in his "Chemical and Physical Geology" that "It is certain that the contents of a lode are of later date than the adjoining rock, so that it can be shown that these contents originate from the adjoining rock, and if it can be ascertained what compounds of the metal exist in that rock, the previously existing minerals may be distinguished from those of later date; but the determination of this point is attended with great difficulties and is generally impossible." It is a well ascertained fact that all the ores of copper are found mostly in crystalline rocks or metamorphic rocks derived from them (some ores however being found in newer formations); and Struve (Ueber die Nachbildung der naturlichen Heilquellen, Heft 2, 17) has proved in the most conclusive manner that granite, porphyry, phonolite, gneiss, basalt, clay slate, trachyte, &c., contained a more or less appreciable amount of sodium chloride, and it is also well known that nearly every spring water flowing from rocks contains sodium chloride or a chloride of one of the alkaline earths (magnesium chloride). Further, it has been shown by Delesse (Jahrb. f. Min. 1862, 605) that all rocks are *saturated* with water, and it is also a well ascertained fact that iron pyrites (FeS_2) is disseminated throughout *all* crystalline rocks. Having these facts before me, I made an experiment in order to ascertain what reactions would occur on heating iron pyrites and a solution of cupric chloride together in a sealed tube at a moderately high temperature. Small pieces of pure iron pyrites were placed in a glass tube, then covered with a moderately strong solution of pure cupric chloride, and the tube sealed up and heated for 14 days at a temperature varying from 135°—210° C., but the greater part of the time the temperature oscillated between 150° and 170°. On the seventh day the colour of the cupric chloride had become considerably lighter, and a small deposit of violet-red crystalline cuprite was observed adhering to the sides of the glass tube, whilst on allowing the tube to cool it was observed that a very considerable amount of cuprous chloride had crystallised out in beautiful transparent colourless tetrahedrons. On the 14th day the tube was opened, as no further changes had taken place, and the contents examined and found to consist principally of cuprous chloride, with a small quantity of unaltered cupric chloride; there being at the same time a good amount of ferric sulphate and cupric sulphate present. The deposit on the glass tube was cuprite. I was unable at the time to make any quantitative determination of the various substances formed in this reaction, and am therefore not prepared to say what the reaction was precisely. Of course the iron pyrites was considerably attacked, but there was no separation of free sulphur; free hydrochloric acid was also present, but not in sufficient quantity to dissolve the small quantity of cuprite formed on the tube. The insoluble crystallised cuprous chloride was carefully washed in distilled water in order to free it from the substances already mentioned, then placed in another glass tube, covered with distilled water, and the tube sealed up and heated for seven days in an air-bath at temperatures varying from 160°—180°. On the second day a very marked amount of a bright-red substance had already formed on the sides of the tube, and here and there minute green spots of a substance somewhat resembling atacamite ($CuCl_2 3CuHO + \chi H_2O$) in colour. This tube was heated at the temperatures given for about two weeks, when it was observed that although the red deposit did not increase in quantity, the cuprous chloride was slowly undergoing decomposition, a black powder having separated out. On opening the tube a smell of hydrochloric acid was perceived. The liquid portion in the tube was poured out, and the solid portion (which principally consisted of unaltered cuprous chloride crystals with a little of the dark-coloured powder) well washed with water. By levigation it was easy to separate the powder from the crystals and to submit it to a further examination. In order to free it completely from cuprous chloride it was heated in a beaker with a solution of ammonium sulphate in water, which dissolved out all the cuprous chloride and left the powder intact; and the latter, on being dissolved in nitric acid, did not give any reaction for chlorine, whilst the presence of copper was very evident: hence there is scarcely any doubt about this substance being cupric oxide. The liquid portion contained free hydrochloric acid and cupric chloride.

When the red deposit first commenced to form upon the sides of the tube a small quantity of violet-red crystalline cuprite was undoubtedly also present, but disappeared on the experiment being continued. I dissolved the red deposit in nitric acid and detected the presence of a large quantity of chlorine in the solution, also that of copper. Therefore I conclude that this red substance is the so-called cuprous oxychloride described by Wöhler (Ann. Chem. Pharm. CXXX. 376). On exposing the substance to the air it takes up oxygen, and acquires a beautiful apple-green colour, becoming a cupric oxychloride which has probably the same composition as Atacamite.

Chalcotrichite.—Being desirous of ascertaining what the action of a moderately strong solution of sodium chloride in water would be upon pure artificially-prepared cuprous oxide when heated with it in a sealed tube, I employed the following method, viz.: The tube was heated in an air-bath for five days at a temperature varying from 150°—180°, when small traces of a green substance (probably atacamite) were observed, the cuprous oxide had become dark-brown in colour, and on the sides of the tube there were small beautiful radiating tufts of an orange-red coloured substance closely resembling the prismatic variety of cuprite called chalcotrichite. Continuing to heat the tube, on the eighteenth day it was opened, when it was found that no further formation of the prismatic substance had taken place, but there was a beautiful deposit of highly crystalline violet-red coloured cuprite on the sides of the tube. The fibrous substance was dissolved in nitric acid (after having been boiled in distilled water for several hours and carefully washed) and tested for chlorine, which was found to be entirely absent, whilst copper was present in large amount: hence there can be no doubt that this prismatic substance was the rare mineral chalcotrichite. From this experiment it is evident that cuprous chloride was formed at first by the reaction between the sodium chloride and cuprous oxide, and this cuprous chloride was decomposed into cuprous oxide again and deposited on the sides of the tube as above described. I shall refer to the importance of the occurrence of chalcotrichite in conjunction with cuprite when I come to consider the origin of all the copper ores in a future paper, and will therefore describe another series of experiments having for their object the direct formation of malachite.

Malachite.—Great difficulty was experienced in carrying out these experiments for any length of time, owing to the frequent bursting of the tubes, due, no doubt, to their being somewhat weakened by the formation of silicates upon the surface of the glass. Pure artificially prepared cuprous oxide was placed in a glass tube and covered with distilled water, saturated with perfectly pure carbonic acid gas, and then sealed up and heated in the air-bath at a temperature varying from 150°—175°. After heating fifteen days, the tube was opened and allowed to stand exposed to the air, when a thin film of malachite was observed coating the small clumps of suboxide here and there. The action of oxygen was found to be absolutely essential in the production of malachite by this method, there being no sign of any such formation in the tube before it was opened. Thus there can be no doubt that the greater part of the malachite formations have been produced by the action of water containing carbonic acid gas in solution upon cuprous oxide, as the malachite of the Gumeschewskoi Mine, near Ekaterinenburg, in Russia, is found deposited in a dark-coloured clay which penetrates a small limestone ridge, the whole formation finally resting upon chloritic slate, I have examined numerous specimens of malachite from all parts of the world, and have always observed strong evidence of the action of water upon them, there being numerous cavities always present, which exhibit deposits of cuprous oxide, or hydrated ferric oxide in their interiors, and occasionally amorphous silica and calcite. Gustav Rose (Mineralogische-geognostische Reise nach dem Ural, dem Altai und dem Kaspischen Meere, 1837), was of opinion after examining the copper formations in the Ural and Altai districts that the cuprite there was formed by direct oxydation of the metal; and, secondly, that the large malachite deposits were formed from the cuprite. This opinion probably well expresses the actual process which eventually resulted in the formation of malachite, as the presence of the clay surrounding the ore, the limestone formation and the hydrated ferric oxide and hornstone (amorphous silica) shows that the former neighbouring crystalline rocks must have been submitted to the powerful action of water, which in all probability contained carbonic acid gas. Rose does not give (so far as I am aware) any experimental proofs of his theory of the formation of the great copper deposits of the districts above mentioned. I considered it therefore necessary to endeavour to determine experimentally the accuracy of his statement. For this purpose I placed perfectly clean polished strips of chemically-pure copper sheet in a glass tube, and covered them with distilled water saturated with carbonic acid gas; the tube was then sealed up and heated in a water-bath at 100° for some time. In three days I observed a decided formation of a film of cuprous oxide (which was crystalline) upon the metal; on the seventh day there were small particles here and there of a green substance, which was undoubtedly a basic copper carbonate—very probably malachite. On continuing the heating of this tube for a few days longer at the same temperature, a considerable amount of cupric oxide was formed in scales, arising, doubtless, from the decomposition of the malachite, as it is a well-known fact that malachite on being boiled in water, decomposes into cupric oxide, carbonic acid gas, and water. In order to see what effect pure distilled water would have upon metallic copper under similar circumstances, I placed a large strip of clean polished sheet-copper in a tube, covered it with distilled water and sealed the tube up, heating it in the water-bath as in the previous experiment. I was astonished to find on the third day that the copper was strongly coated with *cupric* oxide, so that it is evident that pure water has a greater chemical action as an oxidising agent than water containing carbonic acid gas. Having thus obtained the results described, I may be allowed to draw some conclusions from them, as regards the primary copper ore, and the eventual formation of the other ores from it, briefly as follows, viz.:—

1. In all probability the crystalline rocks contain disseminated throughout their mass extremely minute quantities of metallic copper (*when perfectly fresh*); Bischoff mentioning many rocks in which small quantities of cupric oxide have been detected, where its presence would never have been expected.

2. By the action of water, or a solution of carbonic acid gas in water, the metallic copper particles were converted *in situ* into one of the three following substances, viz., cuprous oxide, cupric oxide, and malachite.

3. By the action of water partially charged with sodium chloride (derived from the surrounding rock masses),

cuprous chloride and cupric chloride were formed and carried down into the fissures below, where, by the action of a heat which need not have exceeded 160°, the *cuprous chloride* (for the cupric chloride would be completely reduced to cuprous chloride by the action of the iron pyrites which is universally present in all crystalline and metamorphic rocks) was decomposed into cuprite and cupric oxide and deposited in the fissures or lodes. The ferric sulphate simultaneously formed in the reduction of the cupric chloride would eventually decompose into hydrated ferric acid and accompany the cuprite in the lodes, whilst the free hydrochloric acid formed in this process would readily attack the neighbouring rocks, thus causing the deposition of gelatinous silica in the lodes.

It will now be my next endeavour to study the chemical composition of all the rocks occurring in the immediate neighbourhood of copper mines, and particularly the evidences of decomposition of these rocks at the place of contact with the metalliferous lode, in order to ascertain whether the above conclusions are correct. In a future communication I hope to deal with the statement made by great authorities on this subject, that the sulphides of copper are the primary and oldest compounds of that metal.

"On the Construction of a Room or Series of Rooms free from Germ Life, proposed for use in the performance of Surgical Operations," illustrated by a Model Room and Apparatus, by WILLIAM THOMSON, F.R.S.E.

My object in bringing this paper before the Society is to show what I consider to be a valuable application of a well-known principle, viz.: that of filtering from the ordinary air the innumerable spores which are constantly found floating about in it, and so to arrange a room or series of rooms, in which the air may be rendered optically pure. The practicability of producing, and afterwards retaining in a room or rooms of ordinary dimensions, an optically pure atmosphere, was suggested to me by the results of a large number of experiments which I have carried on during the last seven or eight years, but more especially the results obtained from a series of experiments which I commenced about the beginning of this year, on the cultivation of different fungi, the spores of most of which are found in large numbers, floating about in our atmosphere.

Flour paste suggested itself as the best soil I could obtain on which to grow the different fungi. When flour paste is exposed to the air for a few days, as is well known, fungus makes its appearance, and soon completely covers its surface, and if a number of flour pastes be made and exposed in different places, it will be found that different kinds of fungus spring up, in each showing a splendid variety of different colours, and shades of the same colour. It was with a view to isolate and study these different fungi that one of my last series of experiments was commenced, and it became necessary to look for a means of sowing the seeds of each fungus on a soil which should originally be free from spore life, and afterwards should be effectually protected from the falling of other spores from the air on to the soil, and further, should, if possible, give a free and open supply of air to the microscopic plants.

After a few experiments I found that this could be done effectually by placing flour paste in an ordinary glass shade turned upside down, covering the mouth of the shade with a thin layer of cotton wool placed between two pieces of cloth, the three layers being fixed round the edges of the glass by an elastic band. The paste, when boiled in the vessel thus closed, was afterwards found to remain free from fungus life for any length of time. Having thus been successful by such a rough arrangement in preventing the entrance of fungus spores, I next arranged to try the same experiment on a rougher and larger scale, and consequently had the rough model made which I show here this evening. Each side of this box or room is made of thin layers of cotton wool sewn between two pieces of cloth, with a glass in front, which serves equally as door and window. A conical-shaped piece made of stout paper is pasted to a square frame at its base, which fits against one end of the room. An ordinary glass funnel is placed in this cone with its stem passing through the apex which is firmly tied round it, and an india-rubber tube communicating with a pair of bellows is put on the end of the stem; the large end of the funnel is covered with a thin layer of cotton wool placed between two pieces of cloth, and the whole fixed round the funnel edge by means of an elastic band. When the window at the other end of the room is opened and the bellows worked, a current of air passes through a layer of cotton wool placed between two pieces of cloth on the mouth of the funnel and then through that at the end of the room, moves bodily along the room and escapes at the window, little or no special currents or draughts being apparently produced. This I observed by previously filling the room with fumes. The force of the air, by passing through the two layers of cotton wool, was evidently equally distributed throughout the whole area of the room. My first experiment with this arrangement was made by simply working the bellows for about twenty minutes to endeavour to drive the impure air, with its floating particles and spores, out of the room by passing into it the filtered air from the bellows; then boiling one-half of some flour paste in a basin inside the room for a minute or two, and lastly, closing the window with the glass, the other half of the paste being boiled in a similar basin and left outside exposed to the ordinary or contaminated air. After a few days the difference between the two pastes was very marked, the surface of the one left outside the room was completely covered with fungus, whereas, only 4 or 5 little specks of fungus were observed to be growing on the surface of the one inside the house, which soon developed into 4 or 5 little buttons of fungus. I considered that the spores which had fallen on this flour paste must have been sticking to the sides or roof of the room, and to eliminate this assumed cause of error, I drew out the supporting rods of the room and crushed it up and boiled it in water for half an hour, then allowed it to dry and repeated the same experiments with the two basins of flour paste, the bellows being first worked and the glass then taken out and laid down in the ordinary or contaminated air, then wiped with a clean cloth and put in. In a few days afterwards, the paste outside the house was completely covered with fungus, whilst the one inside was absolutely free from it, and it remained free for nearly a month, when one little speck was observed on it which soon developed into a little button of fungus. This showed that for three weeks after the paste was placed in the room no spore had fallen on it. The only cause of failure I could see was the glass, upon which some spores must have adhered, consequently I had a bag made and sewn on above the window, into which the glass could be pulled in opening the window. When this was done the house was again crushed up and boiled in water, again placed in position, the paste boiled, the window closed, and in this third trial the experiment was perfect. No fungus was developed on the paste inside the house, and after some months it dried up to a hard mass. Thus these thin sheets of cotton wool forming a room of 10,032 cubic inches capacity, *i.e.*, capable of holding 36 gallons of air, was sufficient to prevent the entrance of spores into it, and thus to preserve the flour paste from decomposition during several months, till it had completely dried up.

These results, I think, conclusively shew that ordinary large rooms may be constructed and ventilated with filtered air, by means of fans, so that flour paste, taken as a test standard, would remain in them free from fungus life, and I believe that such a room or series of rooms might be of great advantage in surgery, perhaps as a means of preventing spores from entering the wounds of patients, and so doing away with or lessening the onus put upon the antiseptic treatment, or in giving a better chance of success in serious surgical operations. It is, I understand, believed by many medical men that pus and blood poisoning may be generated in a wound, from the blood corpuscles or bioplasts in the body, and that such developments have nothing to do with the germs of the outside air, and that the antiseptic treatment is employed to assist their development, but no one, so far as I know, has ever proved this beyond doubt, and such an arrangement of a room or rooms as I shall suggest further on would, I believe, be one mode of experimenting, whereby the whole or a great part of a serious obstacle to such researches may be eliminated—but in passing, I should like to mention one slight incident in reference to the development of pus in wounds which would tend to show that certain germs floating in the air are capable either directly or indirectly of developing pus. Some years ago, when working with a solution of albumen, which had been exposed to the air for some time, and which was then putrid, and swarming with bacteriæ, I accidentally cut my finger, and some of the putrid liquid entered the wound. I washed it out as well as possible, and thought nothing more of it. Next morning it was slightly swoollen, and had gathered. I opened it with a knife, and microscopically examined the matter, expecting to find bacteriæ, but to my surprise no bacteriæ could be observed, and all that could be seen was simply pus cells.

The arrangements which I should suggest on the large scale are, a long room or series of rooms in a line, at one end of which should be fitted a fan behind a good filter of cotton wool, a long pipe with a series of bunsen burners set along it, so that when all were lit a line or sheet of flame would be produced which might be gently passed along and made to play on every part of the floor, walls, and roof of the room, beginning near the end at which the fan works and going gradually towards the door; by this means any spores adhering to the walls would be destroyed, and no air could pass back to pollute the walls or floor which had been thus purified. A stove might be arranged at the door end of the room, by which cotton garments to cover the ordinary clothes of the surgeons and attendants might be heated to a temperature presumably sufficient to destroy or paralyse the vitality of any spores which might have been adhering to them, and where the knives and other appliances used might be previously heated, and where water used in washing the wounds might be previously heated under pressure. With such an arrangement at an hospital it seems to me that one interesting mode of investigation into some most important subjects might be commenced.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 5th, 1877.

Charles Bailey, Esq., in the Chair.

Mr. Hurst exhibited some specimens of dried plants from Alexandria, mostly of the Genus Silene.

Mr. Rogers exhibited some specimens of the rare British Moss Bartramidula Wilsoni (B. & S.) from the neighbourhood of Barmouth. It had not been noticed for forty years.

Several other rare mosses were noticed: the more uncommon being Bartramia rigida, and Hednigidium imberbe. Specimens of these were likewise exhibited, with about 25 specimens of Phanerogamic plants, *e.g.*, Dianthus deltoides, Saponaria, Koniga, Veronica hybrida, Mentha 4 specimens, etc.

MR. J. COSMO MELVILL exhibited the new shell of the Genus Rostellaria (R. Martinii, Marrat) from the coral reefs of Cebu, one of the Philippine Islands.

He likewise gave a résumé of the position and natural affinities of the Genus; and produced examples of five other species of Rostellaria from his private collection for comparison. The new species swells the number of known recent species up to nine, including two of the Subgenus Rimella, perhaps referable to Strombus. Its nearest ally is R. curvirostris, (Lum.) from which it differs by its greater size, its lightness, and above all by the channelled suture not being so well developed. It is very strange that so well worked a coast as that of Cebu should at so late an epoch have produced so imposing a novelty, as this is much the largest specimen of the Genus. But two specimens are known; the other having been presented by Mrs. Price Martin to the Liverpool Museum. No other species of Rostellaria proper is known to inhabit the Philippine Isles. Though both the species of the Subgenus Rimella are native there. R. curvirostris is from the Red Sea, the R. rectirostris (Sov.) = fusus (L.) being from Celebes.

Ordinary Meeting, December 11th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

"Note on the Daguerreotype Portrait taken of the late Dr. Dalton," by J. B. DANCER, F.R.A.S.

I was not present at the meeting of this Society on the 27th November, but have read in the printed Proceedings of that meeting that a photographic portrait of the late Dr. Dalton was presented to the Society; and that this photograph was enlarged from a Daguerreotype, taken by the late Mr. John Parry, about the year 1841. I have a doubt as to the correctness of the latter statement, and I will briefly give my reasons for that doubt. I worked at the Daguerreotype from the first announcement of its discovery, and when I came to reside in Manchester, in June, 1841, I still continued to amuse myself and friends with the process. The late Mr. John Parry was an acquaintance of mine, and he was not experimenting in the Daguerreotype at that period; if he had been, I think I should have been aware of it, and especially so if he had taken any photograph of Dr. Dalton. Mr. Beard of London (the gentleman who purchased the patent of the Daguerreotype process for England) opened a photographic gallery on November 18, 1841, in rooms over the Manchester Exchange; and the late Mr. Nicklin was the principal photographer, and through my acquaintance with that gentleman Dr Dalton received an invitation to sit for a photograph. The Dr. did not give his consent willingly, and his intimate friend the late Mr. Peter Clare had much difficulty in getting him down to the gallery. When in the photographic room the Dr. intimated that he would not sit for more than one picture, but Mr. Nicklin contrived to take three portraits. Mr. Peter Clare had one of these; Mr. John Dale the chemist (who had been a pupil of the Doctor's) had another; and I became possessed of the third. Dr. Dalton died in August, 1844, and to the best of my belief the three Daguerreotypes named are the only photographs of the Dr. taken from life. Mr. John Dale has had his Daguerreotype of Dr. Dalton copied and enlarged, and in all probability the retouched photograph at the Society's room has originated from some of these copies. Anything relating to the late Dr. Dalton will, I am sure, interest the members of this Society. I do not hesitate, therefore, to bring this short account of his photographs before the Society. My Daguerreotype of Dr. Dalton is here for the inspection of the members; it has, unfortunately, been roughly handled, through lending it to artists, engravers, sculptors, and seal engravers, and the impression now is faint compared to when it was taken. Among the Daguerreotypes will be found one of the late Mr. Nicklin—it is the centre portrait of the three. This gentleman was one of the photographers sent out by government to the Crimea. He was unfortunately drowned when the Rip van Winkle foundered in the Black Sea. There is also a Daguerreotype of the late Mr. John Parry, which I took about 1848.

Since writing the foregoing, Mr. G. S. Woolley has shown me a small copy of Dr. Dalton's photograph, and on comparing it with my own copy of Mr. Dale's Daguerreotype I find it to be identical. My conjectures have proved correct.

"Note on Metallic Niobium and a new Niobium Chloride," by Professor H. E. Roscoe, F.R.S., &c.

The history of the investigation of the two closely related metals Niobium and Tantalum is of interest as showing how step by step the difficulties and uncertainties which surround the examination of analogous and rare substances such as these are overcome; and I may therefore be allowed shortly to state the growth of our knowledge on this subject.

In the year 1801 Hatchett discovered a new metal in the mineral columbite from Massachusetts, and to this he gave the name of Columbium. Ekeberg a year later found a similar new metal in a rare Swedish mineral, and to it he gave the name Tantalum. Wollaston in 1809 came to the conclusion that these two metals were identical. Berzelius next examined the metal contained in the Swedish tantalite, and afterwards Wöhler found these metals in several other minerals. Heinrich Rose next investigated this subject, but in spite of persistent effort the results of his experiments left the matter in a still less satisfactory condition, inasmuch as he first came to the conclusion that two more new metals were contained in these minerals, to which he gave the names of Niobium and Pelopium, whilst at a later period he decided that both niobic and pelopic acids were different oxides of the same metal, for which he proposed the names niobic and hyponiobic acids.

Then again in 1860 v. Kobell thought that he had found a fourth new metal, which he called Dianium, in the the same minerals, and Hermann believed that he had discovered in them a fifth metal, to which he gave the name Ilmenium. It is to Marignac and Bloomstrand that we owe a deliverance from this state of contradiction and uncertainty. They independently proved that only two metals in reality exist—Niobium and Tantalum—and that these are present in varying proportion in most of the minerals in question. They showed that the compounds derived from Rose's niobic oxide contain mixtures of niobic and tantalic oxides, whilst his hyponiobic oxide is pure niobic oxide.

Bloomstrand arrived at this result by a series of analyses of the chlorides. He proved that the white hyponiobic chloride of Rose is in fact an oxychloride, whilst the yellow niobic chloride contains no oxygen.

Marignac, on the other hand, came to a similar conclusion by an investigation of the fluorides of niobium and tantalum, and pointed out that the oxides are pentoxides Nb_2O_5

and Ta_2O_5, whilst the niobium chloride and oxychloride are represented respectively by the formulæ $NbCl_5$ and $NbOCl_3$.

All the other supposed metals turned out to be either simple mixtures of these two in varying proportions, or such a mixture containing in addition some other metal, such as tungsten, titanium, or iron.

Troost determined the vapour densities of niobium pentachloride and oxychloride, obtaining the numbers 9·6 for the first and 7·87 for the second, agreeing closely with the calculated densities when the atomic weight of niobium is taken to be 94.

Some time ago I obtained several crystals of Greenland columbite which possessed a specific gravity of 5·389 and which contains, as Marignac has shown, little or no tantalum. From this material I prepared niobium pentoxide (Nb_2O_5), niobium pentachloride ($NbCl_5$), and niobium oxychloride ($NbOCl_3$) by well known methods.

I have likewise prepared metallic niobium and a new chloride of niobium. The results of these experiments are contained in the following communication.

The following results of analysis of the pentachloride and oxychloride serve to show the purity of the material.

Pentachloride.

	Calculated.	Found.
Niobium......	34·62	34·38
Chlorine......	65·38	65·55
	100·00	99·93

Oxychloride

	Calculated.	Found.
Niobium......	43·42	42·93
Chlorine......	49·19	49·34
Oxygen (Diff.)	7·39	7·73
	100·00	100·00

Metallic Niobium is difficult to prepare, and precautions similar to those employed for the preparation of metallic vanadium must be taken. It is obtained as a bright steel grey shining metallic crust by passing the vapour of the pentachloride mixed with hydrogen repeatedly through a red-hot glass tube. The metallic crust then deposited on the side of the tube is next strongly ignited in a porcelain tube in a current of hydrogen until no further evolution of hydrochloric acid occurs.

In order to avoid as completely as possible access of traces of oxygen, the very hygroscopic pentachloride was prepared in a narrow tube, and this was transferred bodily into the wider tube in which the preliminary heating with hydrogen takes place.

The metal thus obtained was analysed by placing a weighed quantity in a porcelain boat and heating it in a glass tube in a current of air. The metal takes fire and burns brightly to the pentoxide, a small trace of oxychloride being usually deposited, existing as impurity in the metal. In one instance when the metal was allowed to oxidise very slowly, the formation of an indigo-blue-coloured lower oxide was first observed; this afterwards passed into the yellow pentoxide on further heating.

The following analyses were made with metal obtained in two distinct operations:

I. Weight of Metallic Niobium taken ...... 0·3963 gram.
——— Niobium pentoxide obtained... 0·5480 „

This corresponds to 0·3810 of metal; or the substance contains 96·14 per cent of niobium.

II. Weight of Metallic Niobium taken ...... 0·3900 gram.
——— Niobium pentoxide obtained... 0·5403 „

This corresponds to 0·3790 of metal; or the substance contains 97·18 per cent of niobium.

That the substance thus prepared is not, as has been supposed, a hydride, is shown by the fact that the second of the above specimens on analysis was found to contain only 0·27 per cent of hydrogen.

Metallic niobium possesses a steel-grey colour and bright metallic lustre. It is remarkable that it is not attacked in the cold by hydrochloric or nitric acids, either dilute or concentrated, or even by *aqua regia.* Strong sulphuric acid, on the other hand, dissolves the metal quickly, yielding a colourless solution. The metal does not take fire in chlorine gas at the ordinary temperature, but it does so when gently heated, with formation of pentachloride and a trace of oxychloride, owing to the unavoidable presence of a small quantity of oxygen in the metal experimented with.

The specific gravity of metallic niobium at 15·5° is 7·06 as a mean of two well agreeing determinations made with half a gram of substance.

Niobium Trichloride $NbCl_3$. When the vapour of niobium pentachloride is passed through a heated glass tube it is slowly decomposed, a black deposit of trichloride being formed on the sides of the tube. Niobium trichloride is generally obtained as a black crystalline crust which possesses an almost metallic lustre, and closely resembles in appearance that of a film of sublimed iodine. It is sometimes deposited in long needle-shaped crystals which exhibit a dichroic lustre. The trichloride is non-volatile, it is not deliquescent, nor is it decomposed by water or ammonia, but when brought into contact with dilute nitric acid it is at once converted into niobic and hydrochloric acids. Heated in the air it emits dense white fumes.

For the purpose of analysis the trichloride, from several different preparations, was, in some cases, decomposed by nitric acid directly, and in others first fused with sodium carbonate and the fused mass treated with nitric acid.

	No. 1.	No. 2.	No. 3.
Substance taken	0·2781	0·4406	0·5344
Nb_2O obtained	0·1930	0·3000	0·3611
AgCl	0·5681	0·9333	1·1218

	Calculated.	No. 1	No. 2.	No. 3.
Niobium Nb	46·88	46·68	47·76	47·42
Chlorine Cl_3	53·12	50·53	52·40	51·93
	100·00	99·21	100·16	99·35

When the vapour of niobium oxychloride is passed over red-hot charcoal in an atmosphere of carbon dioxide no change occurs, and the pentachloride is obtained when the carbon dioxide is replaced by chlorine.

A remarkable reaction occurs when niobium trichloride is heated in a current of carbon dioxide; this gas is decomposed, a white sublimate, consisting of oxychloride, is obtained, and the corresponding quantity of carbon monoxide is formed.

That this is the case was ascertained by analysing the oxychloride and by collecting the gas over caustic potash and measuring the volume of insoluble gas which came over. This gas was shown to burn with a blue flame, and the quantity was found in two experiments to correspond closely with the theoretical amount. The following is the composition of the oxychloride thus prepared:—

	Found.
Niobium	43·48
Chlorine	47·03
Oxygen	9·49
	100·00

A decomposition of carbon dioxide by a metallic chloride has hitherto not been observed. Phosphorus trichloride absorbs free oxygen on distillation in the air, with formation of oxychloride, but it does not decompose carbon dioxide.

"On the Retention of Saline Impurities by Hydrated Ferric Oxide," by Mr. CHARLES FREDERICK CROSS, Dalton Scholar in the Owens College. Communicated by Professor H. E. ROSCOE, F.R.S.

It is a fact well-known to all chemists that ferric oxide, when precipitated in the form of hydrate from solutions which contain either organic matter or the fixed alkalis, carries down a certain quantity of these substances in adhesive union.

That this adhesive action is exerted upon *saline* bodies in solution, although an obvious inference, I have found nowhere stated to be the case. The importance of the analytical bearings of such a property, if it do exist, led me to undertake a quantitative investigation of the matter, the results of which I proceed briefly to detail.

I. Two grams fine iron wire were dissolved in HCl, the solution oxidized with HNO_3, the excess of acid driven off by evaporation, distilled water added, and the volume of this solution of ferric chloride made up to about 50 c.c.

The weight of this solution was found to be 56·0955 gram. 1 gram. therefore contains ·0356 gram. Fe as Fe_2Cl_6.

To verify this calculation: 2·529 gram. solution were precipitated by ammonia, and the iron estimated as oxide in the usual way.

	Found.	Calculated.	Difference.
Fe.	·0896	·0901	·0005.

A series of determinations was then made in which the oxide was precipitated by ammonia in presence of sodium chloride, which was selected as a typical saline compound. The results are tabulated below :—

	Weight of Solution taken.	Weight of Fe. contained.	Weight NaCl added.	Weight Fe. from Fe_2O_3 obtained.	Error.	Notes.
a.	2·556	0·0911	2·000	0·0938	·0027	Wash Water 350 cc. pptd. in *boiling* solution.
b.	2·574	0·0917	2·000	0·094	·0023	Wash Water 250 cc. pptd. in *boiling* solution.
c.	2·579	0·0919	1·000	0·0945	·0026	Wash Water 125 cc. pptd. in *boiling* solution.
d.	2·542	0·0906	2·000	0·0952	·0046	Wash Water 175 cc. NaCl added after pption.
e.	2·480	0·0884	2·000	0·0941	·0057	Wash Water 175 cc. pptd. in *cold* solution.
f.	2·587	0·0922	2·000	0·0994	·0072	Wash Water 375 cc. pptd. in *cold* solution.

In the above determinations no more than the ordinary precautions were adopted, *i.e.*, the precipitate was washed with boiling distilled water from 15 to 30 times until the washings no longer gave an alkaline reaction.

In some cases the precipitate of oxide was redissolved after weighing, in HCl, and the iron precipitated as sulphide. The filtrate from the latter was evaporated and the residue ignited—in every case a considerable fixed residue of sodium chloride was obtained.

A similar series of determinations was then made with ferrous ammonium sulphate; weighed quantities were oxidised with nitric acid and ferric oxide precipitated by ammonia in presence of sodium chloride. The results obtained were identical with those already cited. In two of these determinations caustic potash was substituted for ammonia: the results obtained deserve mention:—

II.	Wt. of $Fe(NH_4)_2 2SO_4 6H_2O$.	Per centage Fe theoretical.	Wt. of KOH&NaCl added.	Per centage Fe from Fe_2O_3 obtained.	Error. per cent.	NOTE.
			Grms.			
g.	·706	14·28	5 KOH 5 NaCl	17·93	3·65	Wash water=960 cc. pptd. in *boiling* solution
h.	·720	...	...	18·76	4·48	Wash water=400 cc. pptd. in *boiling* solution.

The precipitates were redissolved in concentrated HCl after weighing. They were observed to effervesce violently on treatment with the acid, and on evaporating the solution the alkaline chloride crystallised out in considerable quantities.

I instance these as indicating the extent of adhesion existing between ferric oxide and the fixed alkalis, and the futility of the practice, which I believe is not unusual, of weighing a precipitate of the oxide from which alumina has been removed by means of potash, without previously redissolving and again precipitating.

Determinations were then made in which special precautions were taken with the view of ascertaining the limit of the error due to the retention of alkaline salt. A weighed quantity of pure iron was taken for each experiment. The results are appended:—

III.	Wt. Fe taken.	NaCl added.	Wt Fe calc. from Fe_2O_3 obtained.	Error.	NOTE.
k.	·1669	0·000	·1666	—0·0003	Washed persistently with boiling water till washing absolutely free from Chlorine.
l.	·2088	1·000	·2096	+0·0008	
m.	·1485	1·000	·1498	+0·0013	

The impurity present in the precipitate in *m* was isolated in the manner described above and weighed. It was found = ·0023 grm. Multiplying this by 0·7, the fraction expressing the ratio $Fe_2 : Fe_2O_3$, we obtain the number 0·0016, which it will be seen is in close agreement with the error obtained (calculated as Fe).

In the following experiment pure iron was brought into solution as Fe_2Cl_6, and precipitated by ammonia alone.

The precipitate was thoroughly washed till the washings were absolutely free from chlorine, and transferred, together with the filter paper, to a retort containing a solution of caustic soda, which had been freed from ammonia by prolonged boiling. The retort was connected with a condenser, the contents of the retort boiled, and the distillate "nesslerised" in successive portions. The result is appended:

	Wt. Fe. taken.	NH_3 estimated in distillate.	Calc. as NH_4Cl.	NOTE.
n.	·2265	·00054	.0017	500 cc. boiling distilled water employed in washing the ppt. of oxide.

Determinations were then made in which other alkaline salts were substituted for sodium chloride:

IV.	Wt. Fe taken.	Salt added. grm.	Wt. Fe. found.	Error.	NOTES.
p.	·160	LiCl. 0·25	·1603	·0003	The isolated impurity was examined with the spectroscope, which showed strong Li line.
q.	·2165	Na_2So_4	·2191	·0026	Washings gave H_2SO_4 reaction after all HCl removed. 500 cc. employed.
r.	·206	KNO_3	·2097	·0037	Wash water=325 cc. ppt. in boiling solution.
s.	·182	KNO_3	·1866	.0046	Wash water =650 cc. pptd. in cold solution.

Having thus sufficiently investigated the adhesive action of ferric hydrate upon salts of the alkalies in solution, determinations were made in which salts of the alkaline earths were substituted for the former. For these experiments it was necessary to employ ammonia free from carbonate, which was prepared by agitating the ordinary solution with lime in a closed flask and filtering just prior to use. Care was taken to add the reagent in but very slight excess. Tne following results were obtained:

V.	Wt. Fe. taken.	Salt added.	Wt. Fe. found.	Error.	NOTES.
t.	·173	$CaCl_2$	·1786	·0056	Pptd. in boiling solution. Wash water =330 cc.
v.	·192	$CaCl_2$	·2052	·0132	Pptd. in cold solution. Wash water = 650cc.

Similar results were obtained in presence of barium chloride. I may mention that by employing an ordinary laboratory solution of ammonia the error on ·152grm. Fe (precipitated in *hot* solution) amounted to ·0072, and on ·2313 (precipitated in *cold* solution) to ·0196 grm. These results were confirmed by isolating the impurities and determining the calcium by precipitation as oxalate. The precipitates effervesced considerably on treatment with the acid.

The above experiments suffice to indicate the magnitude of the obvious error which attends the common practice of separating iron and similar oxides from the alkaline earths by precipitation with excess of alkali.

Since iron is very frequently separated from other metals in the form of the basic acetate, the purity of this compound, *i.e.* of the oxide which results from the washing and ignition of the same, was made the subject of the following determinations:

VI.	Wt. Fe. taken.	Salt added.	Wt. Fe. found.	Error.	NOTES.
w.	·1995	NaCl	·2007	·0012	In all cases solution Fe_2Cl_6 nearly neutralised with $(NH_4)_2CO_3$ and pptd. by $NaC_2H_3O_2$ in excess.
x.	·2106	NaCl	·2122	·0016	
y.	·180	$CaCl_2$	·1825	·0025	Only a trace of Ca could be detected in the isolated impurity.
z.	·1987	$CaCl_2$	·2000	·0013	

Here as before it is seen that alkaline salt is retained by the iron oxide, but the separation of the latter from the alkaline earths is practically complete.

The general conclusions to be drawn from the above results are:

(1). That ferric oxide, when precipitated in presence of salts of the fixed alkalis, removes a certain quantity of the latter from solutions, this quantity being within certain limits independent of the amount of alkaline salt present in solution, but varying with the temperature of the solution at the time of precipitation, being much greater in the case of cold than of hot solutions. (I, IV.)

(2). That the error due to the retention of alkaline salt may, by persistent washing with boiling water, be reduced to a minimum but not completely eliminated. (III.)

(3). That the error is very much greater when the oxide is precipitated by excess of alkali in presence of salts of the alkaline earths; but that the latter may be effectually separated from iron by precipitating this metal in the form of the basic acetate.

(4). That ferrous sulphide does not exert this adhesive action upon salts in solution, and is therefore, where the circumstances of the analysis permit, the best form in which to separate iron from the other metals which accompany it.

In conclusion, I would briefly call attention to the researches of Reichardt and Blumtritt* upon the adhesive action of iron oxide upon gases, as affording indirect evidence in favour of the conclusions at which I have arrived from a consideration of the results of my investigation.

The former chemist found that 100 grm. of ferric hydrate recently prepared by precipitation with ammonia, from a solution of ferric chloride, yielded, under suitable conditions, 375·54 cc. of a gaseous mixture (O,N and CO_2)—that the same weight of hydrate previously washed with hot water yielded 250·82 cc, and that 100 grm. of the oxide after gentle ignition, were found to retain 40 cc. of gas. These results, together with the composition of the gaseous mixtures in each case, is set forth below:

Wt. of Substce.	Condition of oxide experimented upon.	Vol. of gaseous mixture in cc.	Ratio of vol. of substance in that of gas.	Per centage composition of gas by vol.			Wt. of CO_2= *i.e.* per centage.
Grms.				N	O	CO_2	
100	By precipitating Fe_2Cl_6 with NH_4OH and drying in the air.	375·54	1:3	26·29—	3·85—	69·86	·51
„	After washing with hot water.	250·82	1:3·5	18·9—	0·91—	80·19	·39
„	After gentle ignition.	42·69	1:0·58	65.32—	10·54—	24·14	·02

These facts not only explain the fact of the retention of the fixed alkalis and alkaline earths in the form of carbonates, which I have stated to be the case, but compel the inference, apart from experimental verification, that the adhesive action of ferric hydrate upon saline bodies in solution would be equally characteristic.

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* *Journ. Prakt. Chem.*, ii. 440—467 (1866).

Ordinary Meeting, December 26th, 1877.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

"Notice of a large Boulder Stone at Old Trafford, Manchester," by E. W. BINNEY, F.R.S., F.G.S.

Of late years considerable attention has been directed to the large stones found in the till or brick clay about Manchester. Many years since, in vol. viii., Second Series, p. 278 of the Society's Memoirs, a description was given of some of the largest which had been then observed—one of them, found near Mr. Buckley's sand delf at Collyhurst, reached the weight of about 5 tons. Since then a much larger specimen, estimated at nearly 20 tons, has been placed in the public park at Oldham. Probably many others have been met with and escaped my notice.

In the past summer Mr. William Worthington, contractor, of this city, met me in the street and showed me what he termed a piece of a remarkable stone which he had met with in making a main sewer under Seymour-grove, Old Trafford. After looking at it I said, I wish you had taken the stone out for me. He replied, that getting it to the surface would have been no easy task, as it weighed over 50 tons. I went down to Seymour-grove to look for myself, and found plenty of the fragments of the stone, but the specimen itself I did not see, as it had been covered up. The stone was a fine grained gritstone, which Lancashire borers would call a burr. It was remarkably hard, tough and sharp, and its outside was smoothed and scored with parallel striæ as shown by the specimen exhibited to the meeting. Rocks resembling it are sometimes met with in the lower coal measures of Lancashire, but I never saw one so hard, tough, and sharp. Its exterior resembled the skin on the outside of a fine grained sandstone found near a fault and which had been there subjected to great friction, but in the case of the boulder this hardness extended right through the specimen. No traces of bedding or lines of stratification were seen, nor was there evidence of any joints in it. The whole appeared as one compact mass. In breaking it I found black patches which might have been caused by the carbonaceous matter of fossil plants, and thin veins of sparry matter, some of them branching out. Professor W. Thomson, F.R.S.E., of the Royal Institution, has kindly examined the stone for me and finds its specific gravity to be 2·654, water at 60° Fah.=1.

Mr. Worthington has been so kind as to furnish me with the following information:

The Ash, Whalley Range,
Nov. 8th, 1877.

DEAR SIR,

It is impossible for me to give dimensions of the large boulder we cut through in tunnelling for the sewer in Seymour Grove, Old Trafford. I can only form a rough idea of the weight of it. It took two of our men a week to blast through it. We brought to the surface stone which I estimate about fifteen tons. After the opening was cut through it I went down the shaft to examine the same, and found a large mass of stone left below our level and on the westerly side. I should think that we did not get more than one-third of the stone out. There are eight or ten tons of it now lying at my house, which I intend to use for rockery work. Any other information you may require I shall be glad to give you.

Yours truly,

WM. WORTHINGTON.

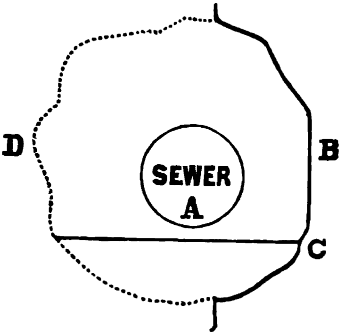
To Mr. E. W. Binney.

Manchester, Nov. 14, 1877.

Large Boulder in Seymour Grove.

DEAR SIR,

I find that we did take out the full depth of the stone, which was something like three feet below our level. The easterly side was also taken off, leaving the bulk of the stone left in on the western side and the roof of the tunnel. The length blasted through horizontally was about ten feet, and the hole driven through it 4ft. 6in. × 3ft. 6in. The section is something like the following:—



To Mr. E. W. Binney.

EXPLANATION.

A—Excavation through the stone for the sewer.

B—Side of the stone cut away.

C—Level of the excavation.

D—The bulk of the stone left in the westerly side and roof of the tunnel, as shown by the jagged line.

The deposit of till in which the stone was found is about twenty feet in thickness, and rests on the pebble beds of the Trias. It is free from many small stones, but a few boulders of moderate size have been met with in making the sewer. The height of the spot where the stone was found is ninety feet above the sea level, the direction of the sewer magnetic north and south, and the longitudinal axis of the stone appeared to be at a right angle to the excavation. The place where it occurred was about a quarter of a mile to the south of the Old Trafford Railway Station, opposite to some dark-coloured palings on the right-hand side of Seymour Grove.

It is hard to say where the stone came from, but it has all the appearance of a fine gritstone of the lower coal measures, and great friction may have generated sufficient heat to alter its structure in a similar manner to what has been done on the sides of great faults and slides where two bodies have been rubbed against each other.

As to its dimensions, we have no data to go upon, except the opinion of Mr. Worthington, an experienced judge of bulks, and a stone merchant, who, when he first mentioned the specimen to me, said that he estimated its weight at over fifty tons. The depth from the surface to the bottom of the sewer was thirteen feet.

"On the Geometrical Representation of the Equation of the Second Degree," by CHARLES CHAMBERS, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. BENNION, F.R.A.S., A.C.P.S.

For every value from $-\infty$ to $+\infty$ of one of the variables in an equation of the second degree between two variables, there are corresponding pairs of values of the other variable; and for every value from $-\infty$ to $+\infty$ of the second variable there are corresponding pairs of values of the first variable. The corresponding pairs of values are of two classes, viz. first those which are not, and secondly those which are, affected by the symbol $\sqrt{-1}$. The Cartesian method gives a perfectly clear geometrical representation of that part of the equation for which the values of both the variables are real, but discards as unintelligible that part for which either of those values is imaginary. In the simplest case—of rectangular coordinates—the unit adopted in the Cartesian system, as the analogue of the unit of the algebraical equation, is a unit length of a mathematical straight line, with a convention as to signs which we need not describe. For a reason that will presently appear, we here make the otherwise simple observation, that if we substitute—in the Cartesian method—the idea of pairs of coincident ordinates and take the product of their values instead of the square of a single ordinate, we only add a convention that leaves the analogy between the algebra and the geometry in question untouched. We further remark that this analogy will still be perfect, if we conceive of the two coincident ordinates as tubes instead of mathematical straight lines, providing the tubes be of infinitesimal transverse section, which may be of circular form and (say) of radius R, and providing further that the numerical values of the tubes are the quantities of curved surface which they possess, the convention as to signs being unaltered: the unit of curved surface must obviously be $2\pi R$. Instead, however, of the curve being described by the motion of the central point of the extremity of the ordinate, let it be described by a diameter, through that point, perpendicular to the plane of the coordinates: it will still be of infinitesimal depth. Further let us take, as the fundamental ideal of the tubular ordinate, the one whose height is R. It may be varied in value by elongation in a positive or negative direction, its diameter remaining constant; or it may be varied by dilatation into larger and larger circles, its height remaining constant: in the latter case the ordinate, which may be called circular to distinguish it from the tubular ordinate, will be of positive sign if it be on the positive side of the coordinate plane, which is perpendicular to the tubular ordinate, and of negative sign if on the opposite side of that plane. Elongation to unit length of the tube, and dilatation to unit radius of the circle, alike make the quantity of curved surface $2\pi R$, which is the unit of this magnitude. Adopting this latter unit as the analogue of the algebraical unit, we shall thus have two distinct but connected systems of variation of the magnitude—that of elongation yielding tubular ordinates analagous to and co-extensive with the ordinates of the Cartesian geometry; and that of dilatation yielding the circular ordinates, the nature of which we now proceed to consider.

Just as in our modified Cartesian system, the ordinate is erected with the axis of the tube at the extremity of the corresponding coordinate, and with a length determined by the equation; so here the circular ordinate must be described (on the plane perpendicular to the tubular ordinate) with its centre at the extremity of the corresponding coordinate, and its radius the function of the latter coordinate given by the equation. Also as the curve, in the Cartesian system, separates the region of greater ordinates than its own at any point from the region of lesser ordinates, so here the curve must be such as will do the same; in other words, it must be the envelope of the series of circular ordinates. The reason will now appear why we substituted the product of a pair of coincident tubular ordinates for the square of a single ordinate: one of the pair we suppose to be produced by the elongation of a fundamental tube, of infinitesimal height and radius, on the positive side of the perpendicular coordinate plane, and the other by elongation of a like tube on the negative side of that plane. The equal dilation of these tubes will produce, not coincident, but adjacent circular ordinates, and these of opposite sign, and of which the product will therefore be a negative square. The curve will be described by the line of intersection of contiguous circular ordinates, that line being of infinitesimal length 2R, half on the positive side and half on the negative side of the plane of the circles.

Once again, we introduce an arbitrary convention that is only additional to, not in contravention of, those of the

Cartesian geometry. In whichever manner the pair of fundamental tubes enlarges, whether by elongation or dilatation, we regard the product of the pair of ordinates produced as the counterpart of the square of the corresponding variable in the algebraical equation. Thus, if γ be the magnitude, regardless of sign, of the geometrical ordinate, then, when $y^2=a^2$, we may for the algebraical y^2 substitute $(+\gamma)(+\gamma)$ or $(-\gamma)(-\gamma)$, and we obtain the result $\gamma=\pm a$; and, when $y^2=-a^2$, we obtain $(+\gamma)(-\gamma)=-a^2$, whence $\gamma=a$. In this way we avoid altogether the introduction of the algebraical symbol $\sqrt{-1}$ as affecting the geometrical representation of the equation. Although in the case of circular ordinates the value $\gamma=a$ is single, it is obvious that two points in the curve—that is, in the envelope of a series of circles whose centres are on a straight line—have the same ordinate and abscissa, one point, namely, on each side of the straight line.

The writer is prepared to have what he has advanced so far regarded as rather curious than instructive, unless it can be shown that the system he describes leads to interesting results. In what follows it will be shown that such results are not wanting.

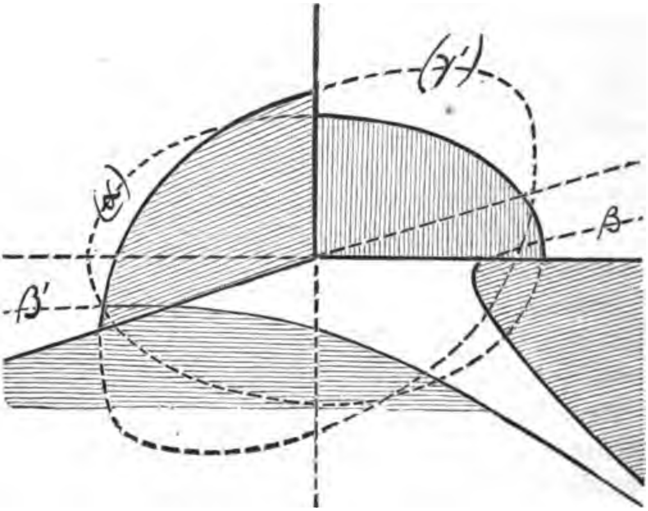
We now write down a series of equations and construct a figure to represent them; and we then proceed to comment on the equations and curves, and on their points of interest in connection with what has gone before.

$$\frac{x^2}{a^2}+\frac{y^2}{b^2}=1 \;\ldots\ldots\ldots\ldots(\alpha) \qquad \frac{x^2}{a^2}+\frac{y^2}{b^2}=-1 \;\ldots\ldots\ldots\ldots(\alpha')$$

$$\frac{x^2}{a^2-b^2}-\frac{z^2}{b^2}=1 \;\ldots\ldots(\beta) \qquad \frac{x^2}{a^2-b^2}-\frac{z^2}{b^2}=-1 \;\ldots\ldots\ldots\ldots(\beta^1)$$

$$-\frac{y^2}{a^2-b^2}-\frac{z^2}{a^2}=1\ldots\ldots(\gamma) \qquad -\frac{y^2}{a^2-b^2}-\frac{z^2}{a}=-1\ldots\ldots\ldots(\gamma')$$

Of equation (α) we have the following cases, a^2 and b^2 being always positive numbers:—



Case 1. When x^2 and y^2 are both positive numbers:— the ordinates are both tubular, and the equation is represented, as in the Cartesian system, by the ellipse (α) whose semi-axes are a and b.

Case 2. When y^2 alone is negative, that is, when x^2 is positive and greater than a^2:—the x ordinates are tubular and the y ordinates circular, and the equation is represented by an envelope on the xz plane, the Cartesian equation to which we shall presently show to be (β), viz:—That of the hyperbola (β). This hyperbola has its vertices in the foci of the ellipse (α), and its foci therefore at the vertices of the ellipse, and its transverse axis is of the same magnitude as that of the ellipse.

Case 3. When x^2 alone is negative, that is, when y^2 is positive and greater than b^2: the y ordinates are tubular and the x ordinates circular: the envelope of the latter has (γ) for its Cartesian equation, and is an impossible curve or in other words, there is no intersection of the circular ordinates and therefore no envelope.

Case 4. When both x^2 and y^2 are negative: the x and y ordinates are both circular, and each pair being compounded (in the manner of the compounding of moments of rotation) forms a single circular ordinate, the plane of which is different from that of every other compound ordinate; hence, there can be no intersection of compound ordinates, and these can have no envelope.

The three equations (α), (β), (γ) possess this remarkable reciprocal property: if any one of them be taken as the equation to be represented, it is itself the Cartesian equation of the corresponding curve when x^2 and y^2 are both positive, and the two others are the Cartesian equations of the respective envelopes when either x^2 or y^2 is alone negative: and thus the ellipse (α) and the hyperbola (β) are the representative curves of all the three equations.

The three similar equations (α'), (β'), (γ'), in which the only difference from the former three is in the reversed sign of unity on the right-hand sides—possess, of course, the same reciprocal property; but here the impossible curve is on the xy plane, that on the xz plane is the conjugate hyperbola to the former one, and is on the same plane with it and has therefore the same asymptotes, whilst the curve on the yz plane is an ellipse whose semi-axes are a and $(a^2—b^2)^{\frac{1}{2}}$.

When $a=b$, that is, (α) is a circle, the three other real curves coincide with the z axes.

To find the Cartesian equation to the envelope of the circular ordinates in the case No. 2, in which y^2 alone is negative, we have for the positive square of the type radius of the circles $\frac{b^2}{a^2}(x^2-a^2)$, and for the equation to a type circle

$$(\xi = x)^2+\zeta^2=\frac{b^2}{a^2}(x^2-a^2) \quad \cdots\cdots\cdots\cdots \quad (1)$$

where x is the variable parameter: hence, taking the first derivative,

$$-(\xi - x) = \frac{b^2}{a^2}x \quad \dots\dots\dots\dots\dots\dots\dots\dots \quad (2)$$

and eliminating x, by means of (1) and (2), we obtain

$$\frac{\xi^2}{a^2 - b^2} - \frac{\zeta^2}{b^2} = 1 \text{ or } \frac{x^2}{a^2 - b^2} - \frac{z^2}{b^2} = 1$$

which is the equation (β) written above. Similarly, by dealing with the impossible ordinates to the y abscissæ, we obtain the equation (γ).

The same three equations (α), (β), (γ), have the reciprocal property that has been described, when, instead of the axes being rectangular, the primitive equation is referred to a pair of conjugate diameters as axes, providing, however, that the tubular ordinates be of elliptical transverse section such that their section by the plane of the two other coordinates is circular.

The extension of the method so as to represent an equation between three variables, referred to rectangular axes, presents no difficulties.

It will be observed that the envelope of the circular ordinates is the same central conic, that we should obtain by taking, in the proper plane, ordinates affected with the sign $\sqrt{-1}$ as normals instead of ordinates and measured from the extremities of the respective abscissæ which yield them.

One interesting consequence of taking account of both the representative curves of an equation, is that it affords a geometrical illustration of the four-point intersection of pairs of conics in cases which are unintelligible without it: in the case of similar and similarly situated concentric ellipses, for example, the absence of any points of intersection on the ellipses is compensated for by the touching of the pair of supplementary hyperbolas on the coincident asymptotes.*

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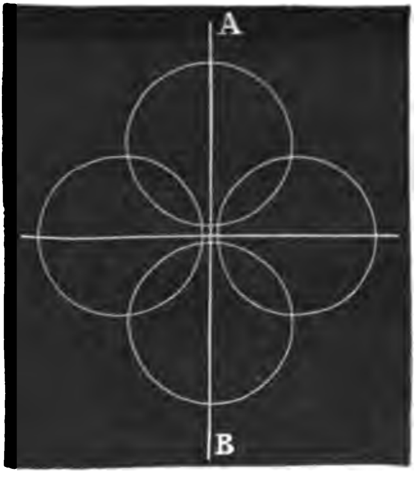
* The writer is under the impression that he will find that, in the general case of elimination between the equations of a pair of conics, if

Years ago, the writer became acquainted with the method in analytical geometry which regards a plane and a straight line—not as the pure abstractions of the mathematician, but—as small portions of the surface of a sphere and of the periphery of a circle respectively in the limits in which the radii of the sphere and circle become infinite; and, whilst noting that these definitions were open to serious objections (chiefly as giving sides to a plane and a straight line of an unsymmetrical character) he was much struck by the extent to which they enlarged the field in which geometrical interpretation might be consistently applied to algebraical forms of expression. He became strongly impressed with the notion that fruitful speculation would lie in the direction indicated—of regarding a plane and a straight line as limits of some kind, and that kind such as would, if possible, cause them, at infinity of distance from the parts considered, to deviate symmetrically from the positions due to the abstract conceptions of a plane and a straight line. The rotation of the system of circles shown on the margin about the straight line AB as an axis will yield examples of a straight line and a



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one or two pairs of impossible values are given for the co-ordinates of the points of intersection, then as many pairs of real values will be given by elimination between the equations of the supplementary pair of conics which lies in the plane that contains the two centres; but he has yet to apply the analytical test of the correctness of this impression. The nature of this test will be to find whether for each case in which the primitive pair of equations gives x^2 or y^2 negative, the supplementary pair gives x^2 or y^2 positive, and *vice versâ.*

plane of the symmetrical kind required, providing that the opposite pairs of equal circles are at the infinitesimal distance R from the central point of the figure, and that their radii are infinities of a second order in comparison with R; for, with these conditions, finite quantities, which are infinite in comparison with R, will be infinitesimal in comparison with the radii of the circles. Let a sphere of finite radius be described with its centre at the central point of the figure, then the part of the tube of which AB is the axis that is cut off by this sphere has the characteristics of our tubular ordinates; and the part of the same sphere cut off by the space between the upper and lower circles forms circles of the nature of our circular ordinates. When the sphere becomes infinite and of radius greater than the diameter of the circles of the figure, the infinite ordinates—tubular aud circular, positive and negative—will all go out into space altogether without the system, in obvious analogy with the sudden passage, in certain otherwise continuous variations, of $+\infty$ and $-\infty$ into each other.

Ordinary Meeting, January 8th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that on the 31st of December last, at about 5.30 p.m., he was walking from Ainsworth to Bury, and he observed a large meteor which started from the star Pollux, and after making a slight curve upwards disappeared near the star Beta in Taurus. It seemed larger than the planet Venus, then brightly shining, and was of a yellowish-white colour, and disappeared without breaking. It left a luminous track of its entire course for a short time after it had passed.

"Note on the Decomposition of Water by Iron-pyrites," by C. A. BURGHARDT, Ph.D.

I have recently endeavoured to ascertain what part iron-pyrites plays in the formation of the natural sulphides of other metals, such as copper-glance, copper-pyrites, &c. These and other sulphides are supposed to arise through the reduction of the corresponding *sulphates* of those metals by organic matter. Bischoff, in his "Chemical and Physical Geology," vol. iii., p. 554, says, "The production of sulphides presupposes the existence of sulphates; and the elimination of sulphur from sulphates, either in combination with hydrogen, or with the metals of the alkalies, or alkaline earths, presupposes the existence of organic life, since that effect would be produced only by carbonaceous substances. The influence of organic substances in the production of sulphides is further shown by the fossil remains, consisting of iron-pyrites, blende, copper-pyrites, variegated copper-pyrites, sulphide of copper and cinnabar." This theory appeared to me to be open to serious objections, as it would be very difficult to show the presence of organic matter in many crystalline rocks sufficient to produce the large sulphide formations often occurring in them through the reduction of sulphates. I therefore desired to ascertain whether sulphuretted-hydrogen could be produced by simpler means, and in accordance with the occurrence of sulphides in rocks *free from organic matter.* With this view I placed some pure finely-powdered iron-pyrites into a glass tube, covered it with perfectly pure distilled water (previously well boiled in order to drive out any air dissolved in it), placed the tube and its contents in a vessel containing boiling water, heated it gently for a short time to drive out any air which might have been contained in the tube, and sealed the latter up. After heating the above in an air-bath for four days, at a temperature of about 120° C., the tube was opened, when a very strong smell of sulphuretted hydrogen was instantly perceived, and proved to be present by its chemical reactions. The liquid contents of the tube were analysed, and found to consist of ferric sulphate, ferrous sulphate, and free sulphuric acid, whilst there was a slight deposit of ferric oxide upon the sides of the glass tube. This experiment certainly proves that sulphuretted hydrogen can be produced in the interior of the earth *without* the presence of organic matter, as all rocks (both crystalline and sedimentary) contain more or less iron-pyrites disseminated throughout them, and are also saturated with water. I hope soon to determine quantitatively the reaction which occurs in the above experiment.

Mr. R. D. DARBISHIRE, F.G.S., exhibited a picture, which he had received from a Japanese gentleman, as one of remarkable excellence and beauty and value in Japan. It represented the Buddhist Trinity seated amidst a court of personages in attitudes of veneration on a complicated open-air platform arrangement, in a sort of park, surrounded with gardens and small edifices or "temples," severally occupied by groups of saints (? or images) and attendants (or worshippers). Above this part of the design was a white region, with groups of personages floating on gorgeous clouds, amidst flying musical instruments and flowers; and above that a deep blue "firmament on high," with similar occupants and enrichments; every floating thing in each space being adorned with waving streamers of various colours.

On the whole platform and park were crowded groups of persons, many distinguished with a saintly nimbus round the head, and some with a flaming one, enveloping the figure from head to foot; the groups apparently representing Buddha and attendants, or various scenes of adoration, or trance, or mere tranquil enjoyment, in houses, gardens, or the water.

On each side of this principal illustration and below it were about fifty separate vignettes of much the same general character as those of the groups first mentioned.

The whole design included many scores of figures, much varied architecture, and a variety of rich conventional decoration, and was brilliantly painted in very fine colours, with a lavish use of gilding for backgrounds and defining lines, with great minuteness and refined taste, upon a piece of very fine white silk, apparently faced with some paper or other substance of the kind, and mounted on a strip of rich brocade, exhibiting in circles, as usual, some Japanese crest.

The picture was stated to represent "What one would see in a Great Buddhist Temple, or in Heaven;" and to have come from a great gentleman's private house.

Mr. D. hoped hereafter to communicate a more detailed description of this very curious picture.

"On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland," by Professor W. C. WILLIAMSON, F.R.S.

In November, 1845, I laid before the Literary and Philosophical Society of Manchester my memoir "On some Microscopic Objects found in the Mud of the Levant and other Deposits; with remarks on the mode of formation of calcareous and infusorial siliceous rocks;" which memoir was published in vol. viii. of the second series of the Society's Transactions. In that memoir I sought to demonstrate two things:—1st, That not only was Chalk made up of microscopic organisms, chiefly Foraminifera, as had recently been demonstrated by Ehrenberg, but that the fact was equally true and explanatory of the origin of all lime-stones except a few freshwater Travertins. 2nd, That some other extensive deposits, of submarine origin, in which no Foraminifera could now be detected, were not in the state in which they were originally accumulated. I concluded that Foraminifera had doubtless been present in them also, but that their calcareous shells had been dissolved out of them, and that this disappearance had been effected through the agency of water containing carbonic acid, at an early stage of the formation of these deposits. As is well known, this latter theory has been reproduced as a new one by some of the naturalists of the Challenger expedition, who have applied it to the explanation of phenomena of a substantially similar nature to those which I endeavoured to account for, in the same way, more than thirty years previously.

I am indebted for the slab of limestone forming the subject of this communication, to my friends the Messrs. Pattison, the marble merchants of Oxford-street, Manchester. This slab appears to illustrate in an exquisite manner both the theories to which I have just referred. It is a specimen of the Bolland limestone, which, when sawn through, was found to contain a large concamerated Nautiloid shell more than 12 inches in diameter, which appears to me to have been a true Nautilus, though the section has not passed exactly through its centre so as to reveal any portion of its siphuncle. In the various parts of this slab we find the calcareous material exhibiting different conditions. Throughout the greater part of its substance we have evidence that it has originated in an accumulation of minute calcareous organisms—especially Foraminifera—but most of these are disintegrated and display vague outlines, a condition which I presume has resulted from the action of the carbonic acid already alluded to.

Scattered through the slab are numerous dark-coloured patches of a substance apparently identical with what the late Dr. Mantell designated Molluskite, and which he believed to be the remains of the soft animal substance of marine organisms. In many of these patches the Foraminiferous shells are better preserved than is the case with the rest of the matrix enclosing the large fossil shell. It appears as if this Molluskite had partially protected the calcareous Foraminifera from the solvent action which had disintegrated most of those forming the rest of the deposit.

But the most interesting features of the specimen are seen within the chambers of the Nautiloid shell. The Foraminiferous ooze has entered freely through the large, open mouth of the terminal chamber in which the animal resided and filled the entire cavity of that chamber. There is no doubt whatever as to the original identity in the character of the ooze thus enclosed within the shell and that which constitutes its investing matrix, though they now appear very different. The latter portion was freely permeated by water containing the solvent carbonic acid; hence the more or less complete disintegration of its Foraminiferous shells. But in the limestone enclosed within the large terminal chamber of the Nautiloid shell, almost every Foraminifer is preserved in the most exquisite perfection. This is especially the case in the deeper part of the chamber, most remote from the mouth, as also in the instances of one or two of the more internal closed chambers, into which the mud has obtained entrance through small accidental fractures in the outer shellwall. It appears obvious to me that the thick calcareous shell of the Nautilus has protected the enclosed shells of the Foraminifera from the action of the solvent acid. I repeat, that there is no room whatever for doubting that the Foraminiferous ooze both contained within and surrounding the Nautiloid shell were originally in identical states. Microscopic observation makes this sufficiently plain. The differences now observable between them have arisen from changes which have taken place subsequent to their primary accumulation, and which changes have been due to differences of position; the one portion has been protected by the thick calcareous Nautiloid shell which would rob the water percolating through it of all its solvent carbonic acid, and thus preserve the contained Protozoa from destruction, and which protection would continue so long as any portion of the Nautiloid shellwall remained undissolved. The other, being unprotected, would be exposed to the full action of the solvent, which would percolate readily amongst the loosely aggregated, microscopic organisms and speedily act upon their fragile shells.

But there is a yet further feature in this interesting specimen requiring notice. The *closed* chambers of the Nautiloid shell are all filled with clear, crystalline, calcareous spar. The acidulated water, acting upon the calcareous Foraminifera of the ooze, has become converted into a more or less saturated solution of bicarbonate of lime. This has passed by percolation through the shell of the Nautilus into its hollow chambers. Finding there suitable cavities, it has gradually filled them up with a crystalline formation of calcareous spar, and which of course exhibits no traces of the minute organisms from which the calcareous matter was primarily derived. A similar crystallisation has filled up the smaller interspaces between the Foraminiferous atoms both enclosed within, and external to the Nautilus, rendering the limestone capable of receiving a high polish.

If these explanations are as correct as I believe them to be, we have here the entire history of the origin of a limestone rock— from the first accumulation of the Foraminiferous ooze, as seen in the interior of the first large chamber of the Nautilus, to the deposition, in an inorganic mineral form, of the crystallized carbonate of lime within the closed chambers of the Nautilus; all being illustrated within the area of a slab of limestone little more than a foot in diameter.

Ordinary Meeting, January 22nd, 1878.

R. ANGUS SMITH, Ph.D., F.R.S., &c., Vice-President, in the Chair.

"On the Cubic Integral $u=\int_\beta^a \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}$ by ROBERT RAWSON, Esq., Assoc. I.N.A., Hon. Mem. Manchester Literary and Philosophical Society; Mem. of the London Math. Society.

1. As far as I know the cubic integral has received but little attention from mathematicians: the reason for this may be that it has been regarded by them as a particular case of the quartic integral.

This, no doubt, is to a certain extent true; still, the quartic integral is readily reduced to the cubic (see Art. 13) and there are some advantages in considering the cubic integral first, in its natural order, especially so in the reduction of it to an elementary integral of a fractional modulus (n) and amplitude (θ).

The quartic integral here alluded to is usually called elliptic integral. I cannot hope, however, to induce mathematicians to abandon this unnatural terminology, viz. *elliptical integrals* for the more natural one of *quartic integrals,* as suggested by Professor Cayley (see Salmon's Higher Algebra, page 83).

In this paper I have not entered into the great question of the comparison of cubic integrals—my effort here has been of a more limited kind, viz., to reduce the cubic integral to the elementary integral

$$u=\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos.m\theta}}$$

Where (n) is a proper fraction, and (m) any positive whole number. I believe this elementary integral is better adapted to the purpose of tabulation than is the elementary integral used by Legendre and others, viz.:

$$\int_0^\theta \frac{d\theta}{\sqrt{1 - k^2 \sin.^2\theta}}$$

to which the former integral can be readily reduced. In tracing the curve whose polar equation is $r^2 = 2 : \sqrt{1 + n\,\text{Cos.}\,m\theta}$ by polar coordinates r, θ the periodicity of cubic integrals is readily perceived.

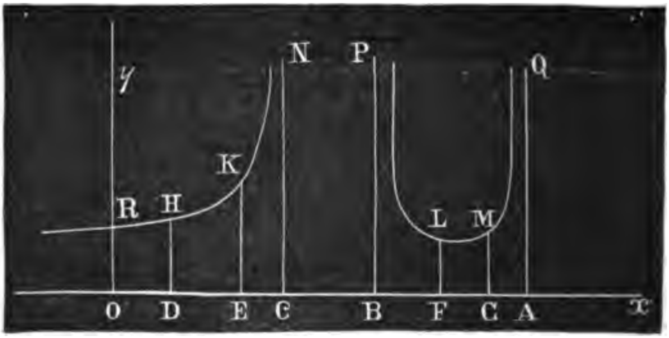
And here I must express my surprise that this method of tracing the cubic integral has not been made use of in tracing elliptic functions. That this has not been done I infer from the circumstance of not finding it in Cayley's recent Treatise on Elliptic Functions.

2. Let a, b, c, be positive, and taken in the order of magnitude, that is $a > b > c$.

If the roots are not positive they can be readily made so by the linear transformation $x = \text{A} + z^2$.

With a view, therefore, of fixing palpably the values of the limits α, β with respect to the roots a, b, c, it will be necessary to trace, by the usual rectangular coordinate method, the curve whose equation is

$$y = \frac{1}{\sqrt{(a-x)(b-x)(c-x)}} \quad \text{.....................} (1)$$



Let Ox, Oy be rectangular coordinates origin at O.

When $x = 0$, then, OR $\sqrt{abc} = 1$. The negative branch of the curve extends to infinity with a continually diminishing value of the positive ordinate. The positive branch extends only as far as A, where OA $= a$. All values of the ordinate (y) beyond this point are impossible. The ordinate (y) is impossible also between B and C, where OB $= b$ and OC $= c$, since the denominator of (1) is then negative.

When, $x = a = \text{OA} \quad \therefore y = \infty$
$x = b = \text{OB} \quad \therefore y = \infty$
$x = c = \text{OC} \quad \therefore y = \infty$
$x < a$ and $> b \quad \therefore y$ is real and positive.
$x < b$ and $> c \quad \therefore y$ is impossible.
$x < c \quad \therefore y$ is real and positive.

In this geometrical representation of the cubic integral there are two cases to consider, viz.:

1. When the limits α and β are both less than c.
2. When the limits α and β lie between A and B.

In case (1) the cubic integral is correctly represented by the area HDEK, where OE $= \alpha$ and OD $= \beta$.

In case (2) the cubic integral is geometrically expressed by the area LFGM, where OG $= \alpha$ and OF $= \beta$.

3. Transformation of the cubic integral (u) into another cubic integral by relation—

$$x = \frac{2c - a + az}{1 + z} = a - \frac{2(a - c)}{1 + z} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

From (2), $dx = \dfrac{2(a - c)}{(1 + z)^2}.dz \quad , \quad a - x = \dfrac{2(a - c)}{1 + z}$

$$b - x = \frac{a + b - 2c}{1 + z}\left\{1 - \frac{(a - b)z}{a + b - 2c}\right\} \quad , \quad c - x = \frac{a - c}{1 + z}(1 - z)$$

The limits become as follows:—

$$\text{When, } x = \alpha \quad \therefore z = \frac{a + \alpha - 2c}{a - \alpha}$$

$$\text{,, } \quad x = \beta \quad \therefore z = \frac{a + \beta - 2c}{a - \beta}$$

Substitute these values in the cubic integral (u) and it becomes

$$u = \sqrt{\frac{2}{a + b - 2c}}\int_{\frac{a + \beta - 2c}{a - \beta}}^{\frac{a + \alpha - 2c}{a - \alpha}} \frac{dz}{\sqrt{(1 - z^2)\left(1 - \dfrac{(a - b)z}{a + b - 2c}\right)}} \quad \ldots\ldots\ldots (3)$$

The value of $\sqrt{a+b-2c}$ is real because $a+b-2c$ is positive.

The fraction $a-b$ upon $a+b-2c$ lies between 1 and zero, it is zero when $a=b$ and 1 when $b=c$. At each of these limits the cubic integral is soluble.

4. Transformation of the cubic integral (3) by the relation

$$-z = \cos. m\theta \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4)$$

Where (m) is any whole positive number.

From (4) $dz = m\sin. m\theta d\theta = m\sqrt{1-z^2}.d\theta$

Substitute these values in (3) and it becomes

$$u = m\sqrt{\frac{2}{a+b-2c}} \int_{\frac{1}{m}\cos^{-1}\left(\frac{2c-a-\beta}{a-\beta}\right)}^{\frac{1}{m}\cos^{-1}\left(\frac{2c-a-\alpha}{a-\alpha}\right)} \frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}.\cos. m\theta}} \quad (5)$$

Let u_1 represent the integral (5) between the limits $\alpha = c$ and $\beta = -\infty$, then

$$\left.\begin{aligned} u_1 &= \int_{-\infty}^{c} \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}} \\ &= m\sqrt{\frac{2}{a+b-2c}} \int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2a}\cos. m\theta}} \end{aligned}\right\} \ldots\ldots\ldots\ldots (6)$$

5. The integral in equation (5) applies only to case (1) in Art. 2; the second case requires a slightly different transformation, as follows:—

Transform the cubic integral (u) into another cubic integral by the relation

$$x = \frac{a+b}{2} + \frac{(a-b)z}{2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (7)$$

From (7), $dx = \frac{a-b}{2}.dz,\ \ a-x = \frac{a-b}{2}(1-z)$

$$-(b-x) = \frac{a-b}{2}(1+z),\ \ -(c-x) = \frac{a+b-2c}{2}\left(1 + \frac{(a-b)z}{a+b-2c}\right)$$

The limits become

$$\text{when } x=\alpha \therefore z=\frac{2\alpha-a-b}{a-b}$$

$$x=\beta \therefore z=\frac{2\beta-a-b}{a-b}$$

Substitute these values in the cubic integral (u), then

$$u=\sqrt{\frac{2}{a+b-2c}}\int_{\frac{2\beta-a-b}{a-b}}^{\frac{2\alpha-a-b}{a-b}}\frac{dz}{\sqrt{(1-z^2)\left(1+\frac{(a-b)z}{a+b-2c}\right)}}\dots\dots\dots(8)$$

6. **Transformation of the cubic integral (8) by the relation**

$$z=\cos. m\theta\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots(9)$$

where (m) is any whole positive number.

From (8) $dz=-m\sin. m\theta d\theta=-m\sqrt{1-z^2}.d\theta$, then (8) becomes

$$u=m\sqrt{\frac{2}{a+b-2c}}\int_{\frac{1}{m}\cos.^{-1}\left(\frac{2\alpha-a-b}{a-b}\right)}^{\frac{1}{m}\cos.^{-1}\left(\frac{2\beta-a-b}{a-b}\right)}\frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}.\cos. m\theta}}\quad(10)$$

Let u_2 represent the integral (10) between the limits $\alpha=a$ and $\beta=b$, then

$$\left.\begin{aligned}u_2&=\int_b^a\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}\\&=m\sqrt{\frac{2}{a+b-2c}}\int_0^{\frac{\pi}{m}}\frac{d\theta}{\sqrt{1+\frac{a-b}{a+b-2c}\cos. m\theta}}\end{aligned}\right\}\dots\dots\dots\dots\dots(11)$$

By comparing (6) and (11) we obtain

$$\int_b^a\frac{dx}{\sqrt{(a-x)(b-x)(c-x)}}=\int_{-\infty}^{c}\frac{dx}{\sqrt{(a-x)(b-x(c-x)}}\dots(12)$$

This result is neat and gives an answer to question 5508 *Educational Times* for December, 1877.

7. Since $2\alpha<2a$, then $2\alpha-a-b<a-b$; therefore $2\alpha-a-b$ upon $a-b$ and $2\beta-a-b$ upon $a-b$ are always proper

fractions whatever may be the values of the real positive roots a, b, c.

The limits of both (5) and (10), integrals which represent the cases 1 and 2 in Art. 2 respectively, are angles θ_1, and θ_2 which lie between zero and (π).

Hence, in both cases referred to in Art. 2, the following integral will apply:

$$u = m\sqrt{\frac{2}{a+b-2c}}\int_{\frac{\theta_2}{m}}^{\frac{\theta_1}{m}} \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \text{ ....(13)}$$

Where $n = \dfrac{a-b}{a+b-2c}$ $\quad \cos.\theta_1 = \dfrac{2c-a-\alpha}{a-\alpha}$ or $\dfrac{2\beta-a-b}{a-b}$

and $\cos\theta_2 = \dfrac{2c-a-\beta}{a-\beta}$ or $\dfrac{2\alpha-a-b}{a-b}$

8. It appears, therefore, that if the elementary integral

$$\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \text{ ....(14)}$$

is calculated, for any given value of the modulus (n), from $\theta =$ zero to $\theta = \dfrac{\pi}{m}$ the result will be a complete solution (arithmetical) of the cubic integral in the case when all the roots are real. The following investigation will show that the same elementary integral will apply equally well when two of the roots are unreal.

9. In the case of the cubic integral having only one real root it assumes the form

$$v = \int_\beta^\alpha \frac{dx}{\sqrt{(a-x)\{(x-b)^2+c^2\}}} \text{ ....(15)}$$

where a, b, c are independent constants. The limits α and β cannot be greater than (a).

Transform the cubic integral (15) by the relation

$$x = \frac{A - B\cos.\frac{m\theta}{2}}{1 - \cos.\frac{m\theta}{2}} \text{ ....(16)}$$

here, $A = a - \sqrt{(a-b)^2+c^2}$; $B = a + \sqrt{(a-b)^2+c^2}$

From (16) $dx = m\sqrt{(a-b)^2+c^2}\,\dfrac{\sqrt{1-\cos.^2\frac{m\theta}{2}}}{\left(1-\cos.\frac{m\theta}{2}\right)}\,d\theta$

$$a - x = \sqrt{(a-b)^2+c^2}\cdot\frac{1+\cos.\frac{m\theta}{2}}{1-\cos.\frac{m\theta}{2}}$$

$$(x-b)^2+c^2 = \frac{\sqrt{(a-b)^2+c^2}\{3\sqrt{(a-b)^2+c^2}+b-a\}}{\left(1-\cos.\frac{m\theta}{2}\right)^2}$$

$$\left\{1+\frac{(\sqrt{(a-b)^2+c^2}+a-b)\text{Cos.}m\theta}{3\sqrt{(a-b)^2+c^2}+b-a}\right\}$$

Substitute these values in (15) observing the change in the limits, then

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2+c^2}+b-a}}\int_{\frac{2}{m}\cos.^{-1}\left(\frac{a-\beta-\sqrt{(a-b)^2+c^2}}{a-\beta+\sqrt{(a-b)^2+c^2}}\right)}^{\frac{2}{m}\cos.^{-1}\left(\frac{a-\alpha-\sqrt{(a-b)^2+c^2}}{a-\alpha+\sqrt{(a-b)^2+c^2}}\right)}$$

$$\frac{d\theta}{\sqrt{1+\dfrac{\sqrt{(a-b)^2+c^2}+a-b}{3\sqrt{(a-b)^2+c^2}+b-a}\,.\cos.m\theta}}\quad\text{....... ....}(17)$$

It is readily seen that $3\sqrt{(a-b)^2+c^2}+b-a$ is a positive quantity whatever may be the values of a, b, c. The coefficient of $\cos.m\theta$ is a proper fraction, therefore, equation (17) may be written as follows:

$$v = \frac{m}{\sqrt{3\sqrt{(a-b)^2+c^2}+b-a}}\int_{\frac{\theta_2}{m}}^{\frac{\theta_1}{m}}\frac{d\theta}{\sqrt{1+n\cos.m\theta}}\text{..................}(18)$$

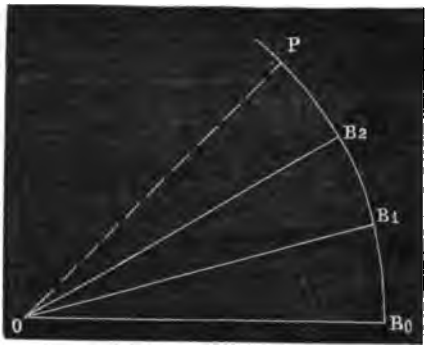
where $n = \dfrac{\sqrt{(a-b)^2+c^2}+a-b}{3\sqrt{(a-b)^2+c^2}+b-a}$, $\text{Cos.}\dfrac{\theta_1}{2} = \dfrac{a-\alpha-\sqrt{(a-b)^2+c^2}}{a-\alpha+\sqrt{(a-b)^2+c^2}}$

and $\text{Cos.}\dfrac{\theta}{2} = \dfrac{a-\beta-\sqrt{(a-b)^2+c^2}}{a-\beta+\sqrt{(a-b)^2+c^2}}$

10. The comparison of the cubic elementary integral, viz.

$$\int_0^\theta \frac{d\theta}{\sqrt{1+n\cos.m\theta}} \quad\dots\dots\dots\dots\dots\dots\dots\dots \quad (19)$$

with elliptic functions of the first order will be considered hereafter. It will be necessary now to show the march of the integral (19) with respect to polar coordinates in the usual way.



Let B_0P be the polar curve traced by the equation

$$r = \frac{\sqrt{2}}{\{1+n\cos.m\theta\}^{\frac{1}{4}}}$$

where $r = OP$ and $\theta = B_0OP$.

To trace the curve B_0P

When, $\theta =$ zero $\dots\dots\therefore OB_0 = \dfrac{\sqrt{2}}{(1+n)^{\frac{1}{4}}}$

" $\theta = \dfrac{\pi}{m} = B_0OB_1\dots\dots\therefore OB_1 = \dfrac{\sqrt{2}}{(1-n)^{\frac{1}{4}}}$

" $\theta = \dfrac{2\pi}{m} = B_0OB_2\dots\dots\therefore OB_2 = \dfrac{\sqrt{2}}{(1+n)^{\frac{1}{4}}}$

" $\theta = \dfrac{3\pi}{m} = B_0OB_3\dots\dots\therefore OB_3 = \dfrac{\sqrt{2}}{(1-n)^{\frac{1}{4}}}$

&c. &c.

It is readily seen that the polar radii from OB_2 to OB_4 are exactly the same as the polar radii included in the angle B^0OB_2, because $\cos.m\left(\dfrac{2\pi}{m}+\phi\right) = \cos.m\phi$.

The polar radii on each side of OB_1 are equal at the same angular amplitude, because $\cos.m\left(\phi+\dfrac{\pi}{m}\right) = \text{Cos}.m\left(\dfrac{\pi}{m}-\phi\right)$

Therefore, the polar area B_0OB_1 is equal to the polar area B_1OB_2. From the above considerations the principle of the periodicity of cubic integrals is readily perceived, and it follows that

$$p\int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{+ n\cos. m\theta}} = \int_0^{\frac{p\pi}{m}} \frac{d\theta}{\sqrt{1 + n\cos. m\theta}}$$

Where, p is any whole number. If the functional notation be used, then

$$pf\left(n\ ,\ \frac{\pi}{m}\right) = f\left(n\ ,\ \frac{p\pi}{m}\right) \ldots \ldots\ldots\ldots\ldots (20)$$

From the figure it will be seen that

$$f\left\{n,\ \left(\frac{2p\pi}{m} + \phi\right)\right\} - f(n,\ \phi) = 2f\left(n,\ \frac{\pi}{m}\right) \ldots\ldots\ldots\ldots (21)$$

$$f\left\{n,\ \left(\frac{2p\pi}{m} - \phi\right)\right\} + f(n,\ \phi) = 2f\left(n,\ \frac{\pi}{m}\right) \ldots\ldots\ldots\ldots (22)$$

$$\therefore f\left\{n,\ \left(\frac{2p\pi}{m} - \phi\right)\right\} + 2f(n,\ \phi) = f\left\{n,\ \left(\frac{2p\pi}{m} + \phi\right)\right\} . (23)$$

where ϕ is less than $\frac{\pi}{m}$.

11. Approximate formula for calculating the cubic integral—

Put $\theta_1 = \frac{\pi}{m}$, r_0, r_1, r_2, &c., the polar radii corresponding to the angles zero, $\frac{\pi}{pm}$, $\frac{2\pi}{pm}$, &c.

It can be readily shown, that

$$\int_0^{\frac{\pi}{m}} \frac{d\theta}{\sqrt{1 + n\cos. m\theta}} = \frac{\pi}{6mp}\left\{r_0^2 + r_p^2 + 4(r_1^2 + r_3^2 + \ldots r_{p-1}^2) + 2(r_2^2 + \ldots r_{p-2}^2)\right\} \quad (24)$$

where (p) is any whole number.

This formula is obtained by supposing the curve B_0P to coincide with the curve whose equation is $r^2 = a_0 + a_1\theta + a_2\theta^2$. The values of r_0, r_1, &c., are as follows :—

$$r_0^2 = \frac{2}{\sqrt{1+n}} \qquad r_1^2 = \frac{2}{\sqrt{1 + n\cos. \frac{\pi}{p}}}$$

$$r_2^2 = \frac{2}{\sqrt{1 + n\cos. \frac{2\pi}{p}}} \qquad r_3^2 = \frac{2}{\sqrt{1 + n\cos. \frac{3\pi}{p}}}$$

$$\vdots \qquad \vdots \qquad \vdots$$

$$r_{p-1}^2 = \frac{2}{\sqrt{1 + n\cos. \frac{\pi}{p}}} \qquad r^2 = \frac{2}{\sqrt{1-n}}$$

The larger p is the more accurate is formula (24).

12. The reduction of the quartic integral, usually called elliptical integral, to the cubic integral is remarkably easy.

To effect this let the quartic integral be as follows:

$$u=\int_{\beta}^{a}\frac{dy}{\sqrt{(e-y)(f-y)(g-y)(h-y)}}\ldots\ldots\ldots\ldots\ldots(25)$$

Where, *e*, *f*, *g*, *h* are in the order of magnitude.

Transform the integral (25) by the relation

$$y=\frac{p+ex}{q+x}=e+\frac{p-eq}{q+x}\ldots\ldots\ldots\ldots\ldots\ldots\ldots(26)$$

From (26) $dy=\frac{eq-p}{(q+x)^2}\cdot dx\,;\ e-y=\frac{eq-p}{q+x}$

$$f-y=\frac{e-f}{q+x}\left(\frac{fq-p}{e-f}-x\right);\ g-y=\frac{e-g}{q+x}\left(\frac{gq-p}{e-g}-x\right)$$

$$h-y=\frac{e-h}{q+x}\left(\frac{hq-p}{e-h}-x\right)$$

Substitute these values in (25), observing the limits, then

$$u=\sqrt{\frac{eq-p}{(e-f)(e-g)(e-h)}}\int_{\frac{\beta q-p}{e-\beta}}^{\frac{aq-p}{e-a}}\frac{dx}{\sqrt{\left(\frac{fq-p}{e-f}-x\right)\left(\frac{gq-p}{e-g}-x\right)\left(\frac{hq-p}{e-h}-x\right)}}\quad(27)$$

This integral only requires that $eq>p$ in order to obtain its arithmetical value.

If $p=0$, then (27) becomes.

$$u=\sqrt{\frac{eq}{(e-f)(e-g)(e-h)}}\int_{\frac{\beta q}{e-\beta}}^{\frac{aq}{e-a}}\frac{dx}{\sqrt{\left(\frac{fq}{e-f}-x\right)\left(\frac{gq}{e-g}-x\right)\left(\frac{hq}{e-h}-x\right)}}\quad(28)$$

which is a cubic integral whose roots can be compared with (1). If the roots *g h* of eq. (25) are impossible, then equation (28) becomes a particular case of the equation considered in art. (9).

If all the roots in eq. 25 are impossible, then it can be readily reduced to the form

$$u=\int_{\beta}^{a}\frac{dy}{\sqrt{(1+y^2)(1+a^2y^2)}}\ldots\ldots\ldots\ldots\ldots(29)$$

Transform (29) by the relation

$$y^2 = \frac{1-\cos. m\theta}{1+\cos. m\theta} \text{..............................} (30)$$

$$\therefore u = \frac{m}{\sqrt{2(1+a^2)}} \int_{\frac{1}{m}\cos.^{-1}\left(\frac{1-\beta^2}{1+\beta^2}\right)}^{\frac{1}{m}\cos.^{-1}\left(\frac{1-a^2}{1+a^2}\right)} \frac{d\theta}{\sqrt{1+\frac{1-a^2}{1+a^2}\cos. m\theta}} \text{........} (31)$$

The equation (31) is, therefore, of the form given in art. (7).

"Notice of a Fossil Plant found at Laxey, in the Isle of Man" by E. W. BINNEY, F.R.S., F.G.S., President.

Many years ago the late Sir William Logan drew attention to the occurrence of fossil plants in the Devonian strata of Canada, and Professor J. W. Dawson, F.R.S., in the Quarterly Journal of the Geological Society, in vols. XV. and XVIII., described and figured some of these specimens. Amongst them was a plant which he designated *Psilophytum*. Dr. S. S. Scoville has since discovered the remains of plants in the Lower Silurians at Longstreet Creek, near Lebanon, Ohio, which Professor Newberry considered as the casts of some large fucoids or marine plants. Count Saporta has found the branch of a fern in the Silurian schists or slates of Angers, France. Professor Leo Lesquereux, to whom we owe so much for his labours in investigating the fossil plants of the United States, in a paper read before the American Philosophical Society, October 10th, 1877, has described and figured a plant from the Lower Helderberg Sandstone, Michigan, under the name of *Psilophytum cornutum*.

In a paper read by myself before this society on the 26th December, 1876, I stated that after some years' search I had not been able to find the *Palæochorda major* mentioned by Professors Harkness and Nicholson as occurring in the Manx schists in such a state of preservation as to be certain of its true nature, but I had a fucoid in my possession found by Mr. Grindlay in the drift near Laxey.

As Mr. Lesquereux's specimen so much resembles the one found at Laxey I shall give his description at length.

"*Psilophytum cornutum, sp. nov. Plate* 1, *fig.* 1.

"Stem thick, dichotomous, divisions variable in distance, the terminal ones short, pointed nearly equal in size and length, surface slightly rugose and irregularly striate.

"The branches in the lower part are thick comparatively to their length, three or four millemetres, irregularly striate when decorticated, or merely punctuate upon the thin bark, with small projecting dots resembling the basilar remains of scales or small decayed leaves; lateral branches short, narrowed to a sharp point; the upper or terminal ones about equal in length, appearing like a pair of pointed horns."

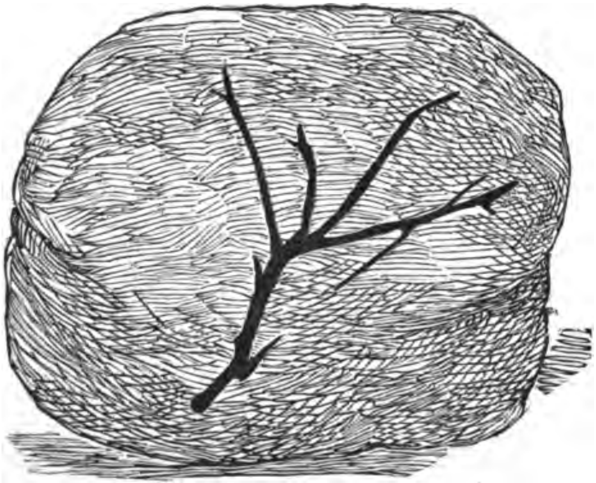
The species is only comparable to some of the fragments not specified but figured by Prof. J. W. Dawson (Geol. Survey of Canada, Fossil Plants of the Devonian and Upper Silurian formations, figs. 243, 244. The author remarks "that these fragments are probably originating in the Upper Silurian of Gaspé; that as they are found in the lower part of the limestone which underlies the Devonian Gaspé Sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land, probably composed of Silurian rocks and supporting vegetation."

From the preservation of its branches even to the smallest subdivisions, the specimens here represent part of a plant embedded in the place of its growth. The matrice is a piece of very hard calcareous shale seven to eight millimetres thick, bearing on one side irregular undulations like ripple marks, without any trace of organic remains, and on the other the fragments of plants as figured here. The branch in (*a*) represents a different species, and indeed a marine or rather a brackish plant, closely related to the species of the present genus *Chorda*, Stack. This fragment seems to have been mixed in the tide pools with fresh water or land plants growing there. For another thick specimen of the same locality and compound, bears a profusion of marine mollusks, and has only branches of this as yet undescribed marine species; *Calamophycus septus.*

Habitat Lower Heldeberg Sandstone, Michigan, discovered and communicated by Dr. Carl Rominger (State Geologist).

On comparing my Manx specimen, which was found on the surface in a field at Laxey, with that figured and described by Prof. Lesquereux; it agrees with the latter in every respect, except that striæ and scales are not observable on the stem. The stem is thick, dichotomous; divisions variable in distance, the terminal ones short, pointed nearly equal in size and length, surface nearly smooth. The branches in the lower part are thick comparatively to their length. The surface of the stem appears to be smooth and affords no evidence of striæ or scales.

The wood cut below represents the specimen a little over the natural size.



The stone in which the plant is embedded is a fine grained grit of a grey colour, and the specimen itself is of a yellow tint, as if coloured by oxide of iron; it runs nearly at right angles to the bedding of the stone, and appears as if standing in the same position as it had grown. The stone is a rolled one but it is evidently from the Manx schists found in the vicinity. These, according to Professors Harkness and Nicholson, are of the age of the Skiddaw slates, but the rock in which the fossil occurs may be of older date, as some of the lower portions of the series have not yet been clearly determined; so here we have evidence of a plant in the lowest part of the Silurian formation, or even lower. By diligent search, the rock in which the specimen occurs may probably be found *in situ* in the upper part of the Laxey valley. The great resemblance, if not identity, of the Manx with the American specimen is very remarkable, and shews the similarity of conditions then prevailing in distant parts of the globe. The specimen might have been called *Psilophytum cornutum*, if any marking on the surface of the stem had been observed, but as these appear to be absent it is proposed to call it *Psilophytum Monense*. As to the nature of the water in which it grew there is no evidence from organic remains, but its characters resemble those of a fucoid more than a land plant.

"On Ptolemy's Geography, with reference to the Coast from Caernarvon to Cumberland," by THOMAS GLAZEBROOK RYLANDS, Esq. Communicated by Dr. R. ANGUS SMITH, F.R.S., &c.

(The author said, This paper was prepared as "A Chip from my Workshop," at the request of a friend who knew that I had been engaged for some years investigating Ptolemy's Geography.)

The special subject of it was selected because it was on the one hand a bit of coast in the far north-west of Ptolemy's habitable world, respecting which he had no unusual information or interest. It therefore supplies a fair test of his accuracy. On the other hand it is the portion of coast best known to ourselves, and it has been the subject of considerable discussion.

A brief review of what the authorities have taught us shows that we have really up to the present time no reliable data upon which to base an opinion.

After examining the statements of Camden, Horsley, the Whitakers, Ormerod, and others, the measures of the true earth and of Ptolemy's world were given, and, it was shown that, so far as the subject of the present paper was concerned, the proportion of six to five might be safely adopted. This was the ratio of the scales adopted by Dr. Whitaker in the earliest years of the century, and by others since.

Commencing at Ianganorum prom: the positions and the errors of each of the stations in succession were examined and determined, with the following results:—

Tisobius fl:	Traeth Mawr.
Ianganorum prom:	Brach y pwll.
Mona.	Isle of Man.
Seteia Est.:	Dee.
Belisama Est.:	Mersey.
Setantiorum portus:	Ribble.
Moricambe Est.:	Morecambe.
Ituna Est.:	Solway.

The application of the name of Tisobius to the Conway was shown to have resulted from the same editorial error which has cut off the promontory of Caernarvonshire in the whole of the editions of the Geography.

The extraordinary width of the Wirrall, and the consequent differences of opinion as to the true name of the Belisama, was proved to have resulted from the fact that the Seteia is placed too low, and not the Belisama too high. This error being corrected, all the stations are consistent to Morecambe.

The contraction of the coast between Moricambe and Ituna was explained and verified by calculation.

The absence of Anglesea from the Geography of Ptolemy, and the fact that his Mona is the Isle of Man "pure and simple" were elucidated and the paper concluded with some remarks upon the value of the several editions of Ptolemy's work.

The erroneous estimates of the size of the world in the early centuries was compared to our own estimates of the solar parallax. The adoption of the mean degree of five hundred stadia implied neither carelessness nor ignorance.

This is not the only correction required, but it is the only one necessary for the elucidation of the "bit of coast" under consideration. To deal with the general subject much more is required. This caution is necessary, as the numbers employed on the present occasion could not be *generally* applied alone.

This is but a "Chip from the Workshop." The whole subject is preparing for publication. In the process of the work it has been found that the so-called blunders attributed to Ptolemy are, almost all of them, capable of satisfactory explanation when his modes of thought and work are understood.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 3rd, 1877.

A. BROTHERS, F.R.A.S., in the Chair.

Mr. **JOHN BOYD** exhibited specimens of the Chiego or "Jigger" Flea (*Pulex penetrans*, L.) from Demerara, and explained its method of attacking the feet, thereby causing them to swell, and unless speedily extricated, being the means of danger, sometimes so great as to render amputation necessary. Magnified representations of the mouth maxillary palpi, mandibles, and proboscis of this species and likewise those of *P. irritans* and *P. felis* were shown, and the various distinctions between the species pointed out. The maxillæ are in *P. penetrans* quite rudimentary; and in other respects it is remarkable for its smallness, the size and strength of its lancets, and its peculiar habit of nidification.

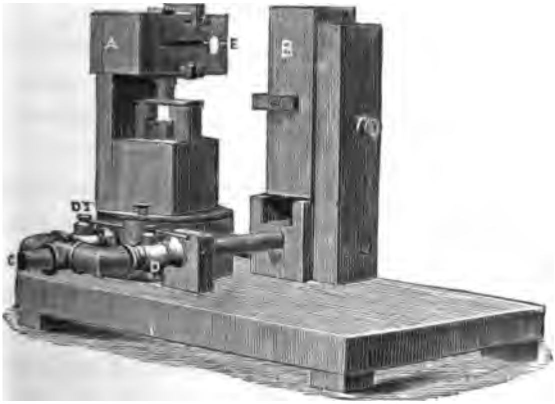
Ordinary Meeting, February 5th, 1878.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Mr. **Thomas Harrison**, F.C.S., exhibited an apparatus devised by Dr. Dixon Mann to obviate the inconvenience attending the use of tuning forks in the projection of Lissajous' figures on the screen; afterwards he reproduced the figures mechanically, by means of his ingenious optical adaptation to Tisley's Harmonograph for showing its movements on the screen.

"On an Improved Method of Projecting Lissajous' Figures on the Screen," by J. **Dixon Mann**, L.K.Q.C.P.

As is well known, the vibrations of tuning forks when used for the production of Lissajous' figures are kept up either by the constant application of the violin bow, or by the aid of an electro-magnet; the former method requiring the presence of two assistants, and the latter adding materially to the complexity of the apparatus and not unfrequently failing to produce the desired result. The difficulty is overcome in the present apparatus by the substitution of harmonium reeds for the tuning forks, the entire instrument being easily controlled by one operator.



The apparatus consists of a base board on which are planted the two reed boxes A and B. The box A is placed horizontally in such a manner as to be capable of slight rotation in the horizontal plane, and also of adjustment in height, by means of the support to which it is attached being provided with a slot and set screw. The box B is permanently attached to the base board in the vertical position. The boxes are so placed that a pencil of light falling directly on E would be reflected to B about one inch from its top; they are furnished with clamping screws for the attachment of the reeds. The boxes are entirely open on the sides facing each other, their margins being covered with soft leather on which the reed plates bed, making a sufficiently air-tight joint. Wind is supplied through the brass tube C which gives off a branch to each box, a stop-cock DD′ being inserted in each branch. The reeds are similar to those used in the construction of harmoniums; they are mounted on brass plates which fit the reed boxes. The tongue of each reed is furnished, at its free end, with a small reflector of microscopic covering-glass (E) silvered by Liebig's process, a piece of cork or pith being interposed between the tongue and the reflector, so as to free the latter from the frame of the reed; the reeds are then tuned in the usual manner. It is not necessary that the reeds should be in absolute tune, as, within certain limits, their relative vibrations can be adjusted by means of the stop-cocks, an advantage of great value, believed to be solely possessed by this apparatus.

The reed in the vertical box gives the fundamental ratio of vibrations from which the intervals are built up. Two fundamental reeds are used interchangeably, one giving the double or eight feet C of musicians, the other, being an octave lower in pitch, adds an octave to the intervals obtained from the first fundamental: thus, the third with the first fundamental becomes the tenth with the sub-fundamental.

The horizontal box is furnished with a set of reeds giving all the intervals up to the twelfth, including the unison. The horizontal reeds are changed for the production of the different figures, the fundamental reed being retained.

It was found, when the wind was allowed to pass directly from the bellows to the reed boxes, that the tongues of the reeds were simply blown past the openings in the reed plates and there sustained, no vibration taking place. To obviate this a free space of half an inch was allowed between the supply pipe and the reed box, so as to afford a cushion of air capable of yielding to the elasticity of the tongue. The supply pipe is contracted at its termination to about one-third the size of the hole in the reed box through which the wind enters.

The apparatus is used as follows:—The base board being firmly clamped to a rigid table, one of the fundamental reeds is clamped in front of the box B; another reed, giving the desired interval, is similarly clamped to the box A; an elastic tube, about half an inch in diameter, is attached at one end to the pipe C, and at the other to an acoustic bellows. A fine pencil of light is now thrown on the mirror E, which is then adjusted by rotation of the box A until the light strikes the mirror of the vertical reed, from whence it is reflected on to a screen of tracing paper placed a short distance away; a condenser, interposed between the lantern and the mirror E, focuses the spot of light on the screen. On the bellows being put in motion the figure appears, and can be brought to a perfect stand in any phase of development, looped or cusped, by careful manipulation of one or other of the cocks. The figures are more distinctly seen on the side of the screen away from the apparatus, the room being in total darkness. It is essential to the perfect production of the figures, especially of those resulting from the more discordant intervals, that the entire apparatus should be as rigid as possible, and free from any vibration other than that produced by the tongues of the reeds, and also that the wind supply should be perfectly steady.

NOTE.—My attention having been drawn by Mr. Baxendell to a paper in the Proceedings of the Royal Society, April 12th, 1877, by Professor Mc.Leod and Lieut. Clarke, "On some figures exhibiting the motion of vibrating bodies, &c.," I think it right to state that my apparatus was constructed in its present complete form in January, 1872, and exhibited to a number of scientific gentlemen in February, 1872; and further, independently of this fact, that the apparatus of Professor Mc.Leod and Lieut. Clarke (so far as is conveyed by the description in the above-mentioned paper) does not affect the originality of my *schema*.—J. D. M.

Ordinary Meeting, February, 19th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

"Note on a Method for Determining the Coefficients in Chemical Equations," by JAMES BOTTOMLEY, D.Sc.

When two or more chemical compounds act upon one another so as to produce new compounds, provided we know the composition of each, the result may be expressed by a chemical equation. Sometimes, in drawing out the equation, we have some trouble to determine the coefficients to be used with the compounds on each side.

As far as I am aware, the methods used are purely tentative. It occurred to me that in cases where the composition of all the products is known, we might apply the method of indeterminate coefficients as follows:—Let S be the symbol of an element and a_1 b_1, &c., the number of atoms of that element occurring in the various compounds entering into the equation.

S_2 and a_2 b_2, &c., have the same meanings with reference to another element. The letters a_1 b_1, a_2 b_2, &c., may have any positive integral value including O. Then a chemical compound may be denoted by $S_1\ a_1\ S_2\ a_2$ &c., which for brevity may be denoted by A. So B will denote a compound of the form $S_1\ b_1\ S_2\ b_2$ &c., and so on for other compounds. Hence the chemical equation may be written in the form

$$P_1A + P_2B + \&c. = Q_1M + Q_2N + \&c. \ldots\ldots\ldots\ldots\ldots (1)$$

P_1, P_2, Q_1, Q_2, &c., being undetermined. Then equating the terms containing S_1 S_2, &c, we have equations of the form

$$S_1(P_1a_1 + P_2b_1 + \&c.) = S_1(Q_1m_1 + Q_2n_1 + \&c.)$$
$$S_2(P_1a_2 + P_2b_2 + \&c) = S_2(Q_1m_2 + Q_2n_2 + \&c.)$$
$$\ldots\ldots\ldots\ldots\ldots\ldots = \ldots\ldots\ldots\ldots\ldots\ldots$$
$$Sn(P_1a_n + P_2b_n + \&c.) = Sn(Q_1m_n + Q_2n_n + \&c.) \cdot$$

From the above equations the letters S_1, S_2, &c., might have been omitted, but are retained to show that there will be as many equations as elements. Hence if the number of elements be equal to the number of compounds we shall have sufficient equations to determine P_1, P_2, Q_1, Q_2, &c. If the number of compounds exceed the number of elements by unity, take any one of the undetermined coefficients, then all the rest may be determined as multiples of that one, and replacing these values in equation (1), both sides will be multiplied by the same arbitrary quantity, which may therefore be omitted.

Within the last few years chemical compounds of considerable complexity have been obtained, and for their quantitative reactions such a method as that suggested might be serviceable. Annexed are two examples of the method applied to well known chemical equations.

I. Copper and nitric acid yield water, copper nitrate, and nitric oxide.

$$P_1Cu + P_2(HNO_3) = Q_1(H_2O) + Q_2(Cu(NO_3)_2) + Q_3(N_2O_2)$$

Hence we have the system of equation

$$P_1 = Q_2,\ P_2 = 2Q_1,\ P_2 = 2Q_2 + 2Q_3,\ 3P_2 = Q_1 + 6Q_2 + 2Q_3.$$

Hence we get $P_2 = \frac{8}{3}P_1$, $Q_1 = \frac{4}{3}P_1$, $Q_2 = P_1$, $Q_3 = \frac{2}{3}P_1$, substituting in the equation and multiplying each side by $\frac{8}{P_1}$ we get the usual equation

$$3Cu + 8HNO_3 = 4H_2O + 2Cu(NO_3)_2 + 2N_2O_3$$

II. Potassium ferrocyanide, water, and sulphuric acid yield ferrous sulphate, ammonium sulphate, potassium sulphate, and carbonic oxide.

$$P_1(C_6N_6K_4Fe) + P_2(H_2O) + P_3(H_2SO_4)$$
$$= Q_1(CO) + Q_2(FeSO_4) + Q_3(NH_4)_2SO_4 + Q_4(K_2SO_4)$$

Here the group SO_4 may be treated as an element. From the above equation we get the subsidiary equations

$6P_1 = Q_1$, $6P_1 = 2Q_3$, $4P_1 = 2Q_4$, $P_1 = Q_2$, $2P_2 + 2P_3 = 8Q_3$, $P_3 = Q_2 + Q_3 + Q_4$.

Hence we get

$$Q_1 = 6P_1,\ \ Q_2 = P_1,\ \ Q_3 = 3P_1,\ \ Q_4 = 2P_1,\ \ P_2 = 6P_1,\ \ P_3 = 6P_1$$

Replacing these values in the equation and dividing both sides by P_1 we get

$$C_6N_6K_4Fe + 6H_2O + 6H_2SO_4 = 6CO + FeSO_4 + 3(NH_4)_2SO_4 + 2(K_2SO_4)$$

which is the equation given by Fownes.

General Meeting, March 5th, 1878.

Dr. R. Angus Smith, F.R.S., &c., Vice-President, in the Chair.

Mr. Andrew Fairgrieve, of 4, Chester Street, Chorlton-on-Medlock, was elected an Ordinary Member of the Society.

Ordinary Meeting, March 5th, 1878.

Dr. R. Angus Smith, F.R.S., &c., Vice-President, in the Chair.

"On the decomposition of Ultramarine by Carbonic Acid," by Mr. S. Sugiura (Student in the Chemical Laboratory of the Owens College). Communicated by Professor Roscoe, F.R.S., &c.

It has been proposed to employ ultramarine for the detection of free hydrochloric acid in presence of carbonic acid, on the ground that it is not acted upon by the latter, while the former easily attacks it.* In making some experiments in connection with this subject, I found however that carbonic acid itself decomposes ultramarine, as the following experiments will show.

A glass cylinder about a foot in height and an inch in diameter was filled with distilled water, and carbon dioxide evolved from sulphuric and sodium bicarbonate or hydrochloric acid and calcspar, was allowed to bubble up through the column of the liquid. In order to be perfectly sure of the purity of carbon dioxide gas, it was passed through a series of flasks containing distilled water, and then through a long tube containing solid sodium bicarbonate, to keep back any free acid which might be mechanically carried over.

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* H. Müller, *Chem. Soc. Jour.*, N. S., viii. 38.

After passing a steady current of the gas through for 15 minutes, the water in the cylinder was tested for acids other than carbonic. The absence of sulphuretted hydrogen was also proved by exposing a paper moistened with lead acetate solution.

Then about ·07 grams of ultramarine infused in 2 cc. of distilled water were added to the water in the cylinder. Within 5 minutes the paper turned brown, showing the presence of sulphuretted hydrogen, which therefore can only be attributed to the decomposition of ultramarine.

In order, however, to ascertain the presence of any alkaline sulphide existing as such and not as a constituent of ultramarine, a large quantity of the latter was treated with distilled water and quickly filtered. The filtrate contained no sulphide. Hence the evolution of sulphuretted hydrogen was really due to the decomposition of ultramarine by the carbonic acid. The colour of the pigment was dulled, but could not be destroyed completely even when metallic chloride existed in the solution.

Through the kindness of Messrs. Jewsbury and Brown, I got a dozen bottles of aerated water, one half of which were charged with 2 grains of sodium chloride in each. The effects of these waters upon ultramarine were compared together week after week by putting ·07 grams of the pigment into the cylinder containing these liquids. In all cases the following effect was observed: the decomposition of ultramarine did not commence at once, but on standing for about an hour the paper moistened with lead acetate solution and placed over the cylinder began to be blackened.

A few minutes afterwards the odour of sulphuretted hydrogen was distinctly observed. To prove the absence of sulphuretted hydrogen in laboratory air, a similar paper was placed over a jar containing the infusion of ultramarine in distilled water, which remained unchanged during the experiments.

These experiments, though qualitative, show that carbon dioxide in aqueous solution, viz. carbonic acid, can attack ultramarine, and therefore the latter is not a safe indicator of free hydrochloric acid in presence of carbonic if the former exist in very small quantity.

"On Siliceous Fossilization," by J. B. HANNAY, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College, communicated by Professor ROSCOE, F.R.S., &c.

My friend Professor Young having mentioned to me that certain questions connected with fossilization were in a very undecided state, and having expressed his opinion that a chemical examination was really what was required to throw light on the question, I obtained from him some siliceous fossils in which he was interested, and of which he and Mr. J. Young, F.G.S., have published an account,* and proceeded to make an examination of them by chemical means. The rods were samples of Hyalonema Smithii, and, as they were in the fossil state siliceous, and probably had been so in the original state, I took for comparison in my examination of them, some rods of a recent siliceous sponge. On examination of the fossil rods it was seen that they might roughly be divided into two classes: I. rods partly transparent or opalescent, and containing some chemically combined water, and II. rods opaque, and apparently consisting of anhydrous crystalline silica, both classes, however, containing many samples stained brown by ferric oxide. Thus I generally experimented with three different kinds, which shall be referred to as

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* Annals of Nat. Hist., Vol. 20, No. 119, P. 425.

I. Recent quite transparent.
II. Fossil opalescent.
III. Fossil opaque.

To find the general differences displayed by the three kinds, several experiments with various chemicals were first performed, placing all the three kinds in the same vessel under the same circumstances. On placing the three kinds in hydrofluoric acid it was seen that the recent rod was dissolved in about 30 minutes, the opalescent in about two hours, and the opaque rod only partially dissolved after four hours immersion in the acid. Sometimes the opaque rod dissolved irregularly, leaving protruberances, and sometimes leaving a spongy substance covering the still undissolved interior. On heating the three kinds in a four per cent. solution of caustic soda in a sealed tube to a temperature of 110° C., so that they were under pressure, the recent rods dissolved in about 10 minutes, No. II. was nearly all dissolved in 30 minutes, leaving only a thin core of anhydrous silica, while after two hours No. III. was only irregularly eroded, and in some places seemed very little affected. On heating the three specimens to 100° with water for two days very little change was observed, in fact, water at the boiling point, was found to be without practical effect, but when heated to 150° C. in a sealed tube, so that it was under a pressure of several atmospheres, the water had the effect of making No. II. much more transparent. It seemed then that at this high pressure that the substance took up water, and so was rendered more similar to the recent sponge rods. I found, however, that if a substance like ether, which boils at a much lower temperature, were introduced into the tube above the water, so that the pressure would increase much more rapidly than with water, the rods might be hydrated at a lower temperature. On heating the rods by themselves No. I. gave off water and became friable sometimes exfoliating, No. II. also gave off water and became opaque (like No. III.) and at the same time friable, while No. III. underwent no change.

The general conclusions to be drawn from these experiments are that the principal difference between the three kinds of rods lies in the relative proportions of water they contain, and that the larger proportion of water contained in any sample, the more transparent is it. From even a cursory examination of the rods, it may be seen that as the proportion of water decreases the original features of the rods become more and more obliterated, so that a totally dehydrated rod is reduced to a mass of crystalline inorganic silica. It will be seen that an examination of the state of combination of the water in the different rods would be likely to throw light on their constitution; but, owing to the small quantity of rods at my disposal, the method for thus examining substances, which I have published elsewhere,* was in this case inapplicable, an optical examination being the method which promised to yield the best results. Heating the rods with caustic soda solution did not give results throwing any light on their structure, the general effect of this reagent being to erode them, as shown in Fig. 1. An attempt was made to gain a knowledge of

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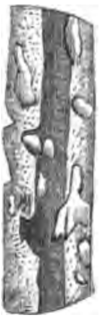

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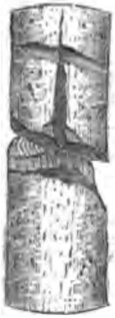

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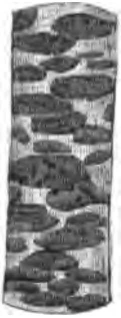

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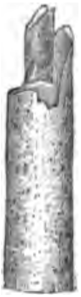

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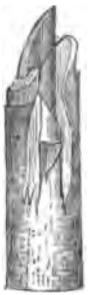

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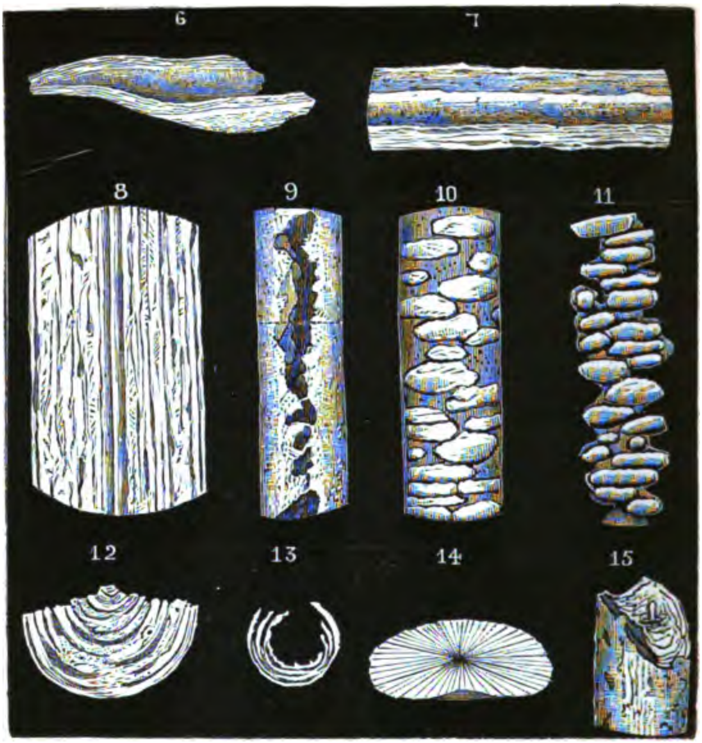
* Examination of Substances by the time method. Chem. Soc. Journ., No. CLXXVII.



the structure of the rods by imbedding a specimen of No. II. in Canada balsam so as to leave only one-half exposed, and then to etch away this upper portion by means of hydrofluoric acid. This method succeeded pretty well, giving a sectional view of the internal structure of the rod, as shown in Fig. 7. The layers are seen by this method to be pretty clearly marked, but the etching is very irregular.

The next method tried was rendering the opalescent nodules transparent by means of high-pressure steam, or rather water, as the substance was always surrounded by water, even when the temperature rose to 150°C. A fractured rod (Fig. 2), thus rendered transparent, shows on the fracture plain markings of the layers composing the rod cylindrically arranged. It was found that when a rod was heated with water in a tube to 150° for five or six hours, the side of the rod next the glass became dissolved, this action seeming to be due to the solvent action of the soda of the glass, as water at that temperature decomposed the glass, removing the alkali. Fig. 9 shows a rod so treated, and Fig. 13 shows a section of that rod through the dotted line in Fig. 9. This action clearly shows at least four layers in the rod. Fig. 15 shows the action of water containing a small quantity of alkali at 150° on a rod the end of which has dissolved to a considerable extent, leaving a hard core of anhydrous silica, which, as my former experiments showed is unacted on by dilute caustic soda.

It occurred to me that if a section were first prepared by grinding or splitting, and this section hydrated as much as possible, and then acted upon by the hydrofluoric acid, it might show the state of the rod, as the hydration of the silica would restore the rod to its original state, and the etching action of the acid would show what that state was.

The experiment was carried on in this way. A rod was split by mechanical means and placed in a thick short tube sealed at one end along with some water. A thin tube much smaller than the other was filled with ammonium fluoride solution, sealed and placed in beside the nodule, and the larger tube was then sealed off and heated in the air bath to 150° C. In rather less than half an hour the ammonium fluoride dissolved its way through the thin tube, and escaping into the larger tube etched the split rod. By these means the silica of the rod was first hydrated and whilst in this state was etched by the fluorine. My reason for wishing the etching to go on after the hydration and without removing the rod or cooling it, was that when the rod is removed from the superheated water it begins immediately to lose its water, that is, the hydration is only temporary. By the above method the rod was not allowed to become dehydrated, but was etched just as the hydration was going on.

Figs. 8 and 12 are specimens obtained by this method, and they show the layer formation to perfection, as many as fifteen layers being shown by one specimen.

Fig. 12 is the end of Fig. 8, and it shows the concentricity of the layers very plainly.

Fig. 6 is a small splinter, the centre of which was in a higher state of hydration than the exterior, so that when treated with hydrofluoric acid it left a hollow interior. The portion left shows the layers also.

I noticed when rendering some of the opalescent rods transparent that they did not all show the same equally distributed transparency, but when viewed by transmitted light dark rounded patches of opaque matter appeared irregularly throughout the transparent portions, as shown by Fig. 3. When viewed by reflected light they showed themselves as white patches upon the opalescent ground, as in Fig. 10. It was found that by heating these rods in dilute caustic soda solution (about 2 per cent.) in sealed tubes to 130° C., the transparent portion could be dissolved away, leaving rounded nodules held together by a portion of the opalescent substance, which, viewed by reflected light, had the appearance given in Fig. 11.

Dr. Young has figured in his paper, rods which are made up of nodules, and which present very much the same appearance as shown in the above figure if the whole of the opalescent portion were dissolved away, and it would appear as though they had been formed in somewhat the same way, that is by the solution of the more highly hydrated portions. The question is, how are the rounded nodules formed in the rods? One of the largest I could find was carefully split and its structure examined, and it was seen when viewed under the microscope by reflected light to have the appearance presented in Fig. 14, that is, it had the character of a radiating nodule of crystalline silica.

Now it appears from the above evidence that in this case of siliceous fossilization the process goes on in something like the following way. The clear transparent rods first lose a portion of their water, becoming transformed into the opalescent or semi-gelatinous-looking form No. II, and in so doing they begin to lose some of their internal structure, that is, the layers adhere more closely and the rods become brittle and lose the glass-like flexibility they originally possessed. At this point any further loss of water causes crystallization to set in, the silica not being able with a lower proportion of water to remain in the "organic" state, and if this crystallization sets in at a great many points the nodule soon becomes an opaque mass of crystalline silica as in variety No. III; but if crystallization sets in at comparatively few places and spreads slowly out, it forms the kind of nodules drawn in Fig. 10. If at this point any solvent agent comes in contact with the rod it dissolves away the non-crystalline portion, leaving nodules as shown by Dr. Young and in Fig. 11; but if no solvent action goes on the rod ultimately becomes transformed into variety III, which is crystalline silica.

The transformation from "organic" to "inorganic" silica is one of arrangement, and as the "organic" arrangement requires a certain amount of water, whenever the proportion falls below this a simpler arrangement sets in, and we have crystalline silica. That the rods of Hyalonema Smithii are the same in constitution at least as those from modern sponges is I think proved from these experiments, and to show a further analogy I have drawn a fractured specimen of such in Figs. 4 and 5, the similarity of which is very striking.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 11th, 1878.

J. BARROW, Esq., in the Chair.

Mr. **BINNEY**, F.R.S., sent a Marine Alga from the Isle of Man for identification. It was not in fruit, but was undoubtedly an unusually narrow fronded form of Chondrus crispus (Lamx.).

Mr. C. **BAILEY**, in the absence of Mr. Hurst, read a paper by the latter "On the Best Method of Collecting and Preserving Plants for Herbarium Purposes, when gathered in Tropical or Subtropical Countries."

Mr. J. **BOYD** exhibited slides of Spongilla fluviatilis, the fresh-water sponge, showing spicules.

PHYSICAL AND MATHEMATICAL SECTION.

December 4th, 1877.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"Transit of the Shadow of Titan across the disc of Saturn, November 23rd, 1877," by **JOSEPH BAXENDELL**, F.R.A.S.

The weather at Birkdale on the evening of the 23rd of November was stormy with thunder, and showers of rain and hail; but between the showers the sky cleared partially and afforded opportunities, at irregular intervals, of observing the shadow of Titan on the disc of the planet. Thirteen estimations of its position at different times during the transit were made, a reduction of which gave 7h. 23·5m. as the Greenwich mean time of mid-transit, or time when the shadow was in the middle of its track; and 3″·9 as its distance from the centre of Saturn's disc at that time. Its path appeared to be on a faint, narrow double belt. The satellite itself was estimated to be on a line perpendicular to the preceding end of the ring at 8h. 17m. All the estimations were made with a power of 180, but the shadow was distinctly visible with a power of 125, the telescope used being an equatorially mounted achromatic of 6 inches aperture by Messrs. T. Cooke and Sons, of York. The frequent interruptions by showers of rain and hail and the disturbed state of the atmosphere rendered it impracticable to use the micrometer with advantage.

February 25th, 1878.

JOSEPH BAXENDELL, F.R.A.S., President of the Section, in the Chair.

"Results and deductions of rain-gauge observations made at Eccles, near Manchester, during the year 1877," by THOMAS MACKERETH, F.R.A.S., F.M.S.

The rainfall of the past year was greatly in excess of the average. The average fall at Eccles for a period of 17 years is 36·123 inches, and the average number of days upon which rain fell for the same period is 208. But the rainfall of last year was 45·175 inches, showing an excess of 9·052 inches or about the average fall of rain for a quarter of a year, and the number of days on which rain fell was 250, showing almost as great an increase in the number of wet days for the year as the excess of rainfall. I have looked through my register of the last 17 years to see if any rule could be observed relating to the excess and deficiency of rainfall. I find that in 13 of those years there are two epochs of dry and wet years, with intervals closely resembling those of the intervals of the sun-spot periods: for example 1865 was a very dry year, having a rainfall of 27·809 inches on 177 days, and the fifth following year, viz, 1870 was the next dry year, having a rainfall of 30·404 inches on 178 days; the next interval is one of three years, viz, 1873, when the rainfall was 31·127 inches on 219 days. Then with regard to the wet years, I find in the same number of years that 1866 was very wet, having a rainfall of 43·076 inches on 232 days, and that the sixth following year was the next very wet one, having a rainfall of 48·416 inches on 264 days. Then after an interval of five years there comes another very wet one, viz., in 1877, when the rainfall was 45·175 inches on 250 days. Thus in an interval of nine years there are three dry years, and in an interval of 12 years there are three wet years.

The following table shows the results obtained from a rain-gauge with a 10in. round receiver placed 3ft. above the ground.

Quarterly Periods.		1877.	Fall in Inches.	Average of 17 years.	Differences.	Quarterly Periods.	
Average of 17 years.	1877.					Average of 17 years.	1877.
52	72	January	5·277	2·940	+2·337	7·784	11·927
		February	4·192	2·337	+1·855		
		March............	2·458	2·457	+0·001		
46	48	April	2·073	1·994	+0·079	6·765	7·028
		May...............	3·059	2·110	+0·949		
		June	1·896	2·661	—0·765		
53	61	July..............	5·518	3·359	+2·159	10·900	12·808
		August	4·880	3·439	+1·441		
		September......	2·410	4·102	—1·692		
57	69	October	4·038	4·216	—0·178	10·724	13·412
		November	5·540	3·448	+2·092		
		December	3·834	3·060	+0·774		
208	250		45·175	36·123	+9·052	36·123	45·175

In the next table are given the results obtained from rain gauges of two different kinds, placed in close proximity in the same plane, and 3ft. from the ground, the one has a 10in. round receiver and the other a 5in. square one. The results, as shown in the table, explain themselves, and show that their monthly and annual differences are extremely small, so that the gauges are good checks upon each other.

1877.	Rainfall in inches in 10in. Round Receiver, 3ft. from the Ground.	Rainfall in inches in 5in. Square Receiver, 3ft. from the Ground.	Differences.	From 1867 to 1877.		Differences.
				Average of 10 years Rainfall in inches, in 10in. Round Receiver, 3ft. from the Ground.	Average of 10 years Rainfall in inches, in 5in. Square Receiver, 3ft. from the Ground.	
January...	5·277	5·297	+·020	3·178	3·228	+·050
February..	4·192	4·212	+·020	2·391	2·374	—·017
March ...	2·458	2·439	—·019	2·375	2·388	+·013
April	2·073	2·011	—·062	2·002	1·983	—·019
May	3·059	3·095	+·036	2·021	1·997	—·024
June	1·896	1·928	+·032	2·511	2·525	+·014
July	5·518	5·624	+·106	3·394	3·377	—·017
August ...	4·880	4·950	+·070	3·545	3·532	—·013
September	2·410	2·408	—·002	3·871	3·829	—·042
October ...	4·038	4·035	—·003	4·689	4·674	—·015
November	5·540	5·447	—·093	3·493	3·508	+·015
December	3·834	3·816	—·018	3·302	3·342	+·040
	45·175	45·262	+·087	36·772	36·757	—·015

In the next table I give the results obtained from two exactly similar gauges, placed at different heights from the ground and free from every interference; each gauge has a 5in. square receiver, and the one is placed 3 feet, and the other 34 feet above the ground. The total fall for the year in the one 3 feet from the ground was 45·217 inches, and in the one 34 feet from the ground was 36·576 inches. The difference between the fall in the two gauges is 8·641 inches, or about 19 per cent. less rain fell in the higher gauge than in the lower. Last year this difference amounted to only 15 per cent., but the moisture of the atmosphere was greater during the past than in the previous year. The table shows that in an average of 10 years the difference amounts to about 18 per cent.

1877.	Rainfall in inches in 5in. square receiver 3ft. from the ground	Rainfall in inches in 5in. square receiver 34ft. from the ground	From 1867 to 1877.	
			Average fall of rain in inches in 5in. square receiv'r 3ft. from the ground	Average fall of rain in inches in 5in. square receiv'r 34ft. from the ground
January	5·297	4·049	3·228	2·319
February	4·212	3·446	2·374	1·836
March	2·394	2·088	2·388	1·899
April	2·011	1·663	1·983	1·660
May..................	3·095	2·635	1·997	1·789
June	1·928	1·536	2·525	2·176
July..................	5·624	4·562	3·377	3·002
August	4·950	4·286	3·532	3·004
September	2·408	2·134	3·829	3·309
October	4·035	3·220	4·674	3·915
November	5·447	4·167	3·508	2·782
December	3·816	2·790	3·342	2·651
	45·217	36·576	36·757	30·342

The following table gives the ratios of the excesses of rainfall 3ft. from the ground over the amount measured at 34ft. from the ground. As I have previously pointed out, no regularity exists in the succession of these ratios during the months of a single year; and if their difference depends upon the relative amount of the humidity of the atmosphere in each month, then it is clear that no regularity for a single year is to be expected. But if an average of six years be considered or the average of any number of years above six, and the following table presents an average of 10 years, then a definite order is manifest. According to this order, December and January are the most humid months of the year, and May and June are the driest. And if the theory be true as to the cause of these differences, then during the past year the atmosphere was driest in the following months, September, March, August, May, April, and February, and was most humid in December, January, November, July, June, and October. The fall of rain in the months when the atmosphere was driest was 19·072 inches, and in the months when it was most moist was 26·103 inches, being a difference of 7·031 inches. In the same table I have given the ratios of this difference or excess of rainfall for 10 years, and placed them in juxtaposition with the mean humidity of the atmosphere calculated from observations of the dry and wet bulb hygrometer, by means of Glaisher's tables. A glance shows that the results are identical, and points almost certainly to the cause of the excess of rainfall between the two gauges, namely, the moisture of the atmosphere.

But that another supposed cause may be fairly discussed I have prepared a table which gives the ratios of the differences of rainfall in comparison with the calculated amount of moisture in the air, and the ratios of the relative amount of the movement of the air for every day of rainfall for the past year. From this table it will be seen that almost in every case the difference of rainfall between the two gauges is as the difference of the moisture of the atmosphere; but if these differences be compared with the ratios of the amount of wind on each day of rainfall, it will be found that there is little or no comparison. For example, though in November there was above the average amount of wind, and an apparent consequent large difference between the rainfall in the two gauges, yet in June and July, when the amount of wind was below the average, a similar difference of rainfall happened; and again, though the amount of wind in May was about equal to that in October, yet there is a great difference in the difference of rainfall.

The wind may play some part in causing this difference of rainfall between the two heights, but I think it is only a secondary cause, that is, when it brings with its force a large supply of relatively moist air. But when it brings a supply of dry air on a rainy day, though there be a strong current of wind blowing, the difference of rainfall will be very small, as illustrated in the amount of rainfall and wind movement in May of the past year.

1877.	Ratios of such Rainfall for 1877.	Ratios of such Rainfall for Average of 10 years from 1867 to 1877.	Mean Humidity of the Atmosphere for 10 years from 1867 to 1877.
January	·764	·718	·872
February..................	·818	·773	·857
March	·872	·795	·833
April	·826	·837	·775
May	·851	·896	·753
June	·796	·861	·743
July	·793	·888	·763
August.....................	·865	·850	·793
September	·886	·864	·779
October	·798	·837	·836
November	·765	·793	·854
December	·731	·793	·873
Annual Ratios......	·808	·825	·810

1877.	Ratios of the Amounts of Rainfall from the previous Table for 1877.	Mean Humidity of the Atmosphere for 1877. 1,000= full Saturation.	Ratios of the Mean Movement of the Wind on the days of Rainfall.
January	·764	·875	175·9
February	·818	·864	156·2
March	·872	·848	111·3
April	·826	·781	99·5
May.................	·851	·744	161·1
June	·796	·704	122·5
July	·793	·822	107·8
August	·865	·813	89·0
September	·886	·782	114·9
October	·798	·821	168·9
November	·765	·846	203·4
December	·731	·883	156·1
Means	·808	·815	138·8

In the next table I give the fall of rain for 1877, during the day from 8 a.m. to 8 p.m., and the fall during the night from 8 p.m. to 8 a.m. From 1868 to 1873 the day fall is always greater than the night fall. In 1874 the night fall was in excess, and in 1875 the day fall was again in excess; and in 1876 and last year the night fall was in excess, but at most, either way, the difference between them is very small.

1877.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	2·719	2·548	—0·201
February	1·626	2·586	+0·960
March...............	0·978	1·461	+0·483
April	1·346	0·665	—0·681
May.................	1·662	1·433	—0·229
June	1·382	0·546	—0·836
July.................	2·551	3·073	+0·522
August	3·107	1·843	—1·264
September	0·857	1·551	+0·694
October	1·446	2·589	+1·143
November	2·675	2·772	+0·097
December	1·399	2·417	+1·018
	21 778	23·484	+1·706

In the next table I present the average day and night fall for a period of ten years. The results of this table continue to show that the day fall exceeds the night fall so far as the whole year is concerned. In the first three months of the year the night fall exceeds the day fall. Then there is the reverse of this from April to July or August, and September and December again show excesses for the night.

AVERAGE OF TEN YEARS, FROM 1867 TO 1877.

1877.	Rainfall in inches from 8 a.m. to 8 p.m.	Rainfall in inches from 8 p.m. to 8 a.m.	Difference between night and day fall.
January	1·486	1·742	+0·256
February	1·023	1·351	+0·328
March...............	1·190	1·198	+0·008
April	1·150	0·833	—0·317
May..................	1·159	0·838	—0·321
June	1·510	1·015	—0·495
July..................	1·782	1·595	—0·187
August	1·772	1·760	—0·012
September	1·756	2·073	+0·317
October	2·378	2·296	—0·082
November	1·785	1·723	—0·062
December	1·423	1·919	+0·496
	18·414	18·343	—0·071

Errata.

Page 96—line 3—*for* $Q_3 = \frac{2}{3}P_1$ *read* $Q_3 = \frac{P_1}{3}$

„ line 6—*for* $2Cu(NO_3)_2 + 2N_2O_2$ *read* $3Cu(NO_3)_2 + N_2O_2$

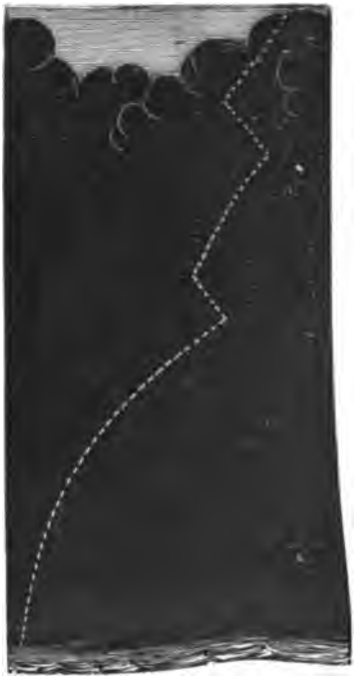
Ordinary Meeting, March 19th, 1878.

E. W. Binney, F.R.S., F.G.S., President, in the Chair.

"On a remarkable Flash of Lightning," by B. St. J. B. Joule, Esq.

On the evening of the 16th of August last year, a heavy thunder-storm took place in this vicinity. It was preceded by a fall of the barometer not exceeding $\frac{1}{10}$ of an inch, the wind at 1 o'clock p.m. being west, backing gradually until at 9 o'clock p.m. it was south. At the time of the storm to which my present observations refer it was south-west, and consequently its direction was nearly parallel with the coast line.

I was standing at the shore end of Leicester Street, watching the approach of the storm, and observing the progress and direction of the more important flashes, when about 8 o'clock a vivid flash of lightning fell apparently into the channel (the water being not much above low water mark of a neap tide) about $\frac{1}{8}$ of a mile north of the end of the pier. In about a minute afterwards another fell about $\frac{1}{8}$ of a mile north-east of the previous one, and after a similar interval a third stream of electricity descended about another $\frac{1}{8}$ of a mile in the same direction. The first and third flashes were of the usual character of forked lightning, but the second presented an appearance which I do not recollect to have witnessed before. From its exit from the clouds to its fall into the sea it seemed composed of small detached fragments which caused it to assume the aspect depicted in the margin.



On the following day, in the course of a conversation respecting the storm of the previous evening, I mentioned the phenomenon to Mr. Thistlethwaite, who informed me that he had been particularly struck by the extraordinary appearance of this singular flash, which he had observed whilst sitting in the "parsonage" (the house adjoining the south-west side of the Manchester and Liverpool District Bank), and which to him appeared exactly as I have depicted it. This gentleman could, however, have seen the upper portion of the flash only, as the houses in Lord Street and on the Promenade intervened between his point of observation and the shore.

Heavy rain seemed to follow in the wake of the third flash, and came on with a noise like that of a great rush of wind, but as the direction of the storm was nearly coincident with the water line, inclining but slightly towards the beach, about ten minutes elapsed before the downfall reached the place where I stood.

From the information I afterwards obtained, the thunderstorm was subsequently, a few miles to the north-east of Southport, more severe than it was in Southport itself.

Southport, 11th March, 1878. B. St. J. B. Joule.

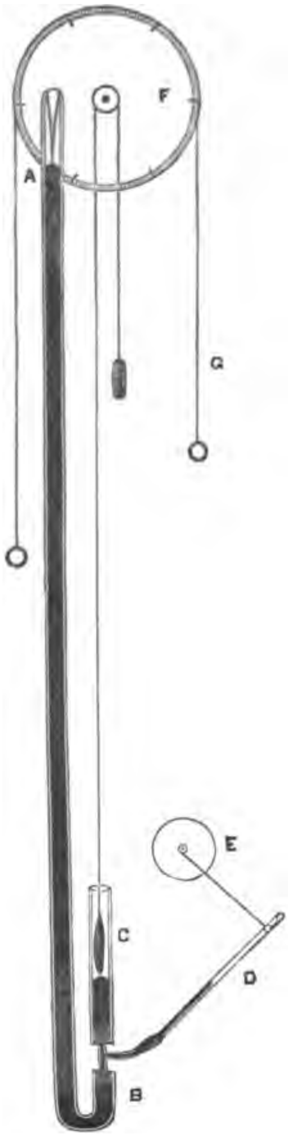
Mr. Baxendell said that Mr. Joule's observation, confirmed as it was by Mr. Thistlethwaite, was very important, as the phenomenon he described was one of extremely rare occurrence; he had himself closely observed many thunderstorms, but had never seen a lightning flash similar to the one depicted by Mr. Joule.

The President said he had seen a similar discharge at Saddleworth about 30 years ago.

"On a Barometer," by Dr. J. P. Joule, F.R.S., &c.

Some years ago I brought under the notice of the Society a syphon barometer, the peculiarity of which consisted in the introduction of a small quantity of sulphuric acid. I hoped that the diminution of the capillary effect, and the extreme mobility of the mercury thus obtained, would present some advantages. I found however that the opinion expressed by Dr. R. A. Smith, viz., that the acid would act on the mercury, was fully justified in the event; for the barometer, after a few weeks had elapsed, stood at too low a level, and now, after an interval of 5 years, stands 3 inches below its proper height, while a plentiful crop of transparent prismatic crystals has been formed.

In the barometer which I now venture to submit, I have applied a principle which I have found very useful in the construction of the manometer. Sealed within to the top of the long leg of a syphon barometer is a piece of thin glass rod, with the extremity of which at A the mercurial column can be brought into contact by means of the adjunct to the lower short leg. C is a tube of the same diameter as the rest of the barometer: it is connected with the short leg by means of a T tube, to which also the narrow glass tube D is attached by a piece of rubber tubing. By raising this small tube by turning the axle E, the mercury can be brought to touch the glass point at A. Affixed to the upper part of the framework of the instrument is a graduated wheel F having a groove in its periphery and also a small V groove near its centre. This last holds a fine wire with a glass plummet at one end and an exact counterpoise at the other. To bring this plummet in contact with the mercury of the lower leg, the wheel is moved by means of the thread G. The contacts of the mercurial column are observed with microscopes, and the value of the graduations of the wheel by comparisons with a standard rule.



In order to facilitate the application of the needful temperature corrections, it is desirable to secure the upper part of the barometer to the block of timber which supports the wheel, leaving the lower end free. There is also an advantage in introducing a small quantity of sulphuric acid into the small tube D, to promote the freedom of movement of the mercury in it.

The average time occupied by an observation is found to be $\frac{3}{4}$ of a minute, and the average error $\frac{1}{1300}$ of an inch. In the morning of yesterday the wind was high and gusty, causing the mercury to oscillate at short intervals of time through a space of about $\frac{1}{200}$ of an inch.

Mr. C. H. STEARN exhibited and described a new form of portable Sprengel pump. In this arrangement all atmospheric pressure is removed from the mercury in the upper reservoir, and the lower receiver exhausted by a mechanical pump before the Sprengel is set in action. In consequence of the reduction of pressure on the reservoirs the length of fall tubes can be diminished to only a few inches; the time occupied in exhaustion is also greatly shortened as the preliminary exhaustion of the receiver removes at the same time most of the air from the vessels attached to the upper part of the pump. In the earlier form of the instrument exhibited by the author at Plymouth the fall tube was single; in the present modification this was divided into three branches, as in Mr. Giningham's pump, and far greater rapidity was obtained in consequence. Exhaustions of 0·0005 mm. have been obtained with these pumps, and the spark from a 5-inch induction coil has been completely arrested by a space of 3 mm. in the vacuum. A modification of M'Leod's gauge was also described, adapted for use with the portable Sprengel.

"A Comparison of the Standard Barometer of the Owens College Physical Laboratory with the working Barometer," by Mr. MORISABRO HIRAOKA, Student of Owens College. Communicated by Professor B. STEWART, LL.D., F.R.S.

The standard barometer tube was constructed in 1876, by Mr. Hicks, according to the plan of the late John Welsh. It has an internal diameter of about 33 millimeters. It stands on a solid block of stone, and is supported by a cast-iron framework, which was made at the workshop of Owens College.

The cathetometer, with which the height of the barometer is read, has a brass scale divided into millimeters, which reads to ·02 mm. by a vernier. It stands on another block of stone, and its distance from the barometer is about 3·5 meters. It was made by Oertling, of London.

The working barometer is of the form usually known as a Portable Standard, constructed by Casella. It is furnished with a brass scale divided into both inches and millimeters, which reads to ·002 inches and ·05 mm. respectively by a vernier. It has been verified at the Kew Observatory, and reported to be correct to the third place of decimals in inches and to have a correction to metrical scale = —·05 mm.

Preliminary determinations.

I. Comparison of the cathetometer scale with the standard yard, placed at the same distance as the barometer.—The standard yard is made of brass, and it has been verified and found correct at the Kew Observatory. From the mean of seventeen readings it was found that 1000 divisions of the cathetometer scale is equal to 999·88 mm. (the probable error =±·01 mm.). In the reduction of inches into millimetres 1 apparent inch was assumed to be equal to 25·3916 apparent mm. when the two scales were at the same temperature.

II. Comparison of the dividing machine with the standard yard and the determination of the length of the barometer screwhead pointer by the former.—From the mean of ten readings its length was found to be =111·340 mm. (the probable error =±·001 mm.).

III. Comparison of the English and metrical scales of the working barometer.—If both scales are correct at the respective standard temperatures, one apparent inch should be equal to 25·3916 apparent mm. $(=\frac{25{\cdot}39954}{1+16{\cdot}67a}$ where $a={\cdot}0000187)$ at any common temperature. From the mean of the following comparisons it was found that ·052 has to be subtracted from the reading of the metrical scale to make it correct, which agrees well with the correction given in the Kew certificate, namely—·05.

Inches.	MM. (Calculated) (1)	MM. (Observed mean) (2)	MM. Difference (2)—(1)
28	710·965	711·006	+ ·041
28·5	723·661	723·720	+ ·059
29	736·356	736·400	+ ·044
29·5	749·052	749·110	+ ·058
30	761·748	761·817	+ ·069
30·5	774·444	774·500	+ ·056
31	787·140	787·180	+ ·040
			+ ·052

The actual comparison of the two barometers.

The mode of procedure. This was done in the following order:—

A Take reading of the upper surface of mercury of the standard barometer.

B Take reading of the working barometer.

C Adjust the screw-head of the standard.

D Repeat *A*.

E Repeat *B*.

F Note temperatures.

G Read the height of the centre of cross of the screw-head (twice).

The temperature was observed by the thermometer affixed to the working barometer, another plunged in mercury of the standard, and the third hung in contact with the cathetometer scale. The last thermometer was carefully shielded from the direct radiation from the body of the observer.

Results (all corrections mad

	Standard B.	Working B.	Difference.
1.	775·21	775·35	+0·14
2.	775·39	775·44	+0·05
3.	775·28	775·17	−0·11
4.	775·07	774·93	−0·14
5.	775·54	775·82	+0·28
6.	774·63	774·80	+0·17
7.	774·25	774·24	−0·01
8.	774·24	774·34	+0·10
9.	773·54	773·67	+0·13
10.	773·85	773·91	+0·06
11.	775·12	775·07	−0·05
12.	775·06	775·11	+0·05
13.	775·24	775·18	−0·06
14.	774·98	775·01	+0·03
15.	774·69	774·78	+0·09
16.	774·56	774·38	−0·18
17.	774·39	774·49	+0·10
18.	774·52	774·52	0·00
19.	759·87	759·86	−0·01
20.	759·90	760·03	+0·13
21.	759·99	759·95	−0·04
22.	760·37	760·22	−0·15
(Mean)	772·077	772·103	+0·026 mm.
In inches	30·3972	30·3982	+0·0010 in.

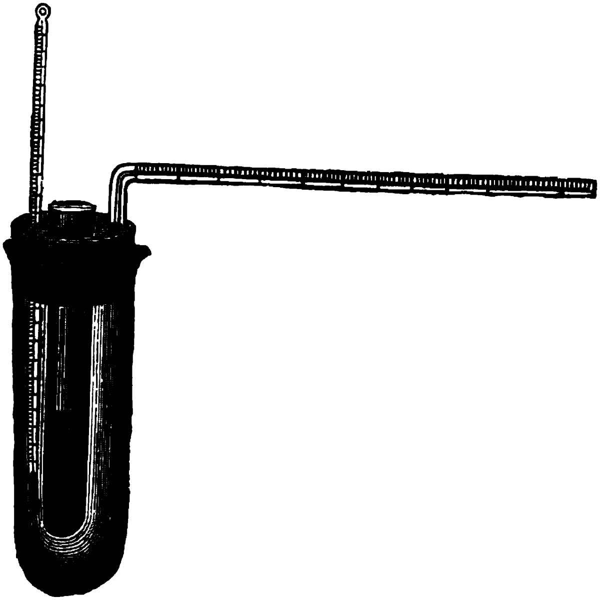
If to the above results, a small correction due to the difference in height of the cisterns of the two barometers (the cistern of the working barometer being about 40 mm. lower than that of the standard), the difference becomes +0·022 mm. or less than +0·0009 inches.

The object of the experiments described in the present paper is not so much to find out the absolute difference of the two instruments employed, as to show the degree of accuracy to be attained by an ordinary amount of care in this mode of comparing standard barometers at different places. Although every precaution was taken to ensure accuracy as much as possible, still there were many other sources of error, especially probable want of uniformity of temperature in different parts of the instruments, and also too great distance (about 12ft.) between the standard barometer and the cathetometer, which was unavoidable in the present arrangement of the Laboratory. So that, as far as these observations go, the standard barometer of the Laboratory and that of the Kew Observatory may be considered to agree, within a small experimental error.

"On a New Calorimeter," by J. B. **Hannay**, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

Having been desirous some time since of making a number of calorimetric determinations, I set about constructing an instrument for the work, but I found that working in the rapid, irregular manner in which I was compelled to do by using only odd leisure moments, the difficulty of obtaining a perfectly constant temperature was very great; and as I have in a great measure overcome that difficulty, I think that an account of the instruments which I have constructed may be of some use to workers who may be placed in such a position as mine, and may desire to make measurements quickly, and without waiting until the temperature is quite constant. In the first place it is evident that as the external or accidental variation of temperature of the instrument is generally not more than 0·2 to 0·3 of a degree; if the internal variation be made greater in one case than another, the error arising from accidental variation will be smaller. Now if we use a liquid body to expand as an indicator we must, if we want a large rise of temperature, use a small quantity of the liquid or a large quantity of the body experimented with; either of these conditions, however, involves difficulties of a kind nearly as bad as the temperature variations; but if we use a gas as our index, that is to say, if we use an air vessel into the middle of which we plunge the substance whose specific heat we wish to determine, and have the vessel surrounded by the usual non-conducting material, we can have a large rise of temperature in a considerable space of matter. Of course at the present time, when the specific heats of substances such as silver, copper, and iron, have been very accurately determined, we do not need to have a water or absolute calorimeter, but only an instrument which will accurately register the rise of temperature and the value of whose graduations may be determined by standard metals; so that although we do not know the absolute calorimetric value of the heat communicated to the air and glass of such an instrument, yet if its indications are proportional to the amount of heat communicated, we have all that is required to form a relative calorimeter. Then as to the construction of such an instrument. It was found that the use of ground joints in the instrument might be entirely avoided, as a red india-rubber stopper can be adjusted with nearly as much accuracy as a ground joint.

The apparatus shown below as Fig. I. is the first form of

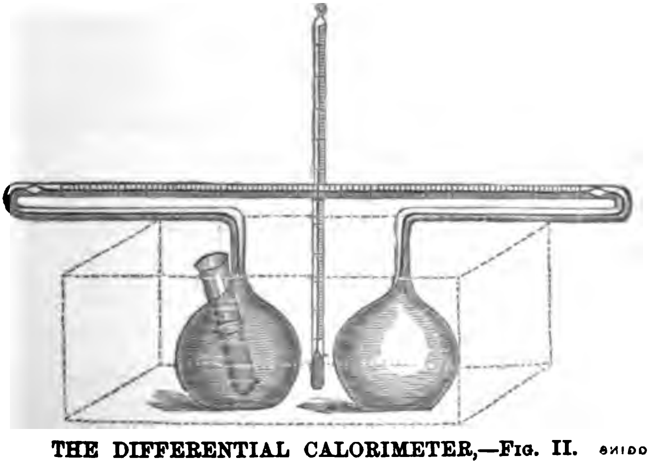


THE NEW CALORIMETER.—FIG. I.

the calorimeter I constructed. A ring was etched near the top of the wide outer tube, which was then filled with water, and the india-rubber stopper pressed in till it was level with the ring (the excess of water escaping by the index tube) and the whole weighed. This was repeated several times, and it was found that (provided the temperature were constant) the weights differed only by two or three centigrammes at the most. If, however, the stopper were permanently fixed in and the index tube and thermometer and interior tube pushed into their proper marks afterwards the weights differed only by a few milligrams. It was thus seen that the volume of gas in the apparatus could be adjusted with as great accuracy as by ground stoppers and with less chance of leakage.

The apparatus consists then of a wide outer tube having a horizontal ring etched round it (in case it may be necessary to replace the stopper) and fitted with an india-rubber stopper through which the thermometer, reception tube and index are inserted. The reception tube may also have a ring etched round it, and be filled with water or other liquid to that mark, in order to cause any non-conducting body to give up its heat quickly to the glass, but I generally prefer to use the apparatus dry and stop the end of the reception tube with cork in order to prevent convection currents. When this latter method is used the apparatus is always ready for use, and requires no other preparation than placing the index in position, which is managed as follows:—With regard to the substance of the index I have found mercury not sufficiently mobile, so I have used instead a coloured aqueous solution preferably containing a little glycerine. The calorimeter is firmly clamped and enclosed in wool, and a small drop of coloured liquid is made to touch the end of the index tube, when it is at once drawn in. The number on the scale of the thermometer level with the stopper is read off, and the thermometer, which has been slightly greased, is pulled up (screwing it from side to side to make it rise easily) till the coloured liquid is drawn up to zero on the scale, then the number of the thermometer stem level with the stopper is again read off. We have then the volume of the scale tube in terms of degrees on the thermometer, and so whenever a calorimetric determination is about to be made the thermometer is pushed down that amount beyond its standard level, and just before a determination is made the index is introduced and sucked in (by means of the thermometer) to zero. It will thus be seen that the calorimeter is open to the air and in a state of equilibrium until the experiment is about to be made, and the index being adjusted by this simple contrivance involves no special parts for the purpose. As this calorimeter can be made and used by any student, I have found it extremely useful in inculcating the principles of thermal chemistry.

It naturally occurred to me that by connecting the end of the index tube with another air vessel which would be kept in the same conditions as the calorimeter vessel, we would have an instrument in which the external or accidental variations affecting both sides of the system would in no way affect the equilibrium, and the index would only be moved by any difference affecting only one vessel, or in other words we would have a differential calorimeter. The apparatus drawn as Fig. II. shows the construction of such an



THE DIFFERENTIAL CALORIMETER,—FIG. II. FIGGINS instrument. Two glass bulbs—one holding an interior tube for the reception of the substances, and the other a little smaller in diameter in order that its cubic contents may be about the same—are fitted with an index tube of small bore, as shown in the figure, which is graduated between the two small cavities on the upper portion of the tube—the cavities being there to prevent the index being accidentally drawn into either of the bulbs. The two bulbs are placed in a box (indicated by dotted lines) surrounded by non-conducting material, and the instrument is ready at any time for use. A thermometer is placed between the bulbs in order that the temperature may be noted at which the experiment was tried. In the instrument No. I. the indications are nearly proportional to the specific heat of the substance, but in the second or differential calorimeter a curve should be drawn of the values for known metals just as is done for the spectroscopic scale, and the specific heats of the bodies reduced by reference to the curve. To show the accuracy of the instruments I give here some specific heats taken at random from many I have done.

First, by No. 1 Calorimeter:—

Substance.	Found	Correct Numbers.
Arsenic	·0820	·0814
Tin	·0575	·05623
Platinum	·0339	·0335
Bismuth	·0315	·03084

I should say that I took as my standards to draw the reference curve the following metals:—

Lead	·03065
Silver	·05701
Zinc	·09555
Iron	·11380
Magnesium	·2499

The Differential Calorimeter gave:—

Substance.	Found	Correct Numbers.
Arsenic	·0818	·0814
Tin	·0565	·05623
Platinum	·0338	·0335
Bismuth	·0309	·03084

In conclusion, I would say that the form of air calorimeter I have given as No. 1 is very well adapted for teaching, as it enables students to construct for themselves instruments of sufficient exactitude to yield numbers very closely agreeing with standard numbers, while for calorimetric investigations, where freedom from all error is the first requisite, the differential calorimeter is very well adapted. The latter may also be made with india-rubber joints, but, as there is pressure, I prefer to have the joints hermetically sealed. In determining the heat of combination by these instruments the same relative method is used as for specific heats. It will be obvious that one great advantage in these instruments is the very small amount of substance required to give the necessary deflection. I have found in practise that from 0·5 to 2 grammes is quite sufficient, the amount, of course, varying with the size of the instrument.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 11th, 1878.

Charles Bailey, Esq., in the Chair.

"On Bryozoa," by Arthur Wm. Waters, F.G.S.

When we meet with the term zoophytes it does not now give an idea of scientific accuracy, but we do not often think how literal a signification it had when first employed. The zoophytes have included organisms in many widely separated groups, as the Anthozoa, the Hydrozoa, Bryozoa, Sponges, and even the calcareous Algæ, and these were at one time held by some of the school of Pallas to be, as the word implies, plant-animals, and this in a very real and extraordinary manner, for they maintained that the branching stalks were of a vegetable nature, containing organisms of an animal nature. The theory of Pallas was supported by Linnè and opposed by Ellis, and in the discussions on this question the Bryozoa were used as much as any of the other zoophytes to show the vegetable nature of what we now know to be an integral part of this animal, which, together with other zoophytes previously to Ferato, had been thought to be merely plants.*

Since then the Actinozoa have been separated, including animals, having one simple body cavity and divided by radiating calcareous or non-calcareous divisions; and the Bryozoa were shown by Milne Edwards, 1828; Thompson, 1830; Ehrenberg, 1832, to vary considerably from the Hydrozoa in having a curved digestive system with two orifices: the first called them tunicated polyps, the second Polyzoa, and the third Bryozoa (moss animals). Since then additions have been made to our knowledge of these animals, and there have been many battles fought over them, and as there are many points unsettled, we may expect "wars and rumours of wars" for a long time.

Many of the Hydrozoa in the external forms of their seaweed-like growth closely resemble the Bryozoa, but differ in having only a single cavity, and, unlike the Anthozoa, have no radial divisions.

The calcareous Algæ are now under the care of the botanists, and do not require any further consideration from us.

The economy of the animals now under consideration is in many respects as strange as anything in the animal kingdom, and we shall see that there are many points which show an external resemblance to plants. As mentioned, the general form is often very closely that of the seaweed, and the polypide, which was called the flower, is not formed until the zooecium* is moderately developed; then after the polypide has enjoyed life for a time it disappears by decay and resorption, and in time another polypide buds out from the inside of the same cell, and thus it was held that the "flower" was deciduous. Again, a very large number throw out radical tubes and fibers. Some have a large number at the base, as in Cellaria ceroides, others from all parts of the stalk, and from specimens of Diachoris I find there is one root from the back of each zooecium (cell). That these points were used to maintain their animal plant nature, when but little was known of their anatomy, cannot be wondered at, and we may see why Pallas† says "Zoophyta esse animalia vere vegetantia, in plantæ formam excresentia; plantarumque alias quoque proprietates affectantia; esse plantas quasi animatas."

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* For the history of the Bryozoa see Johnstone's Brit. Zoophytes; Dr. Allman, Freshwater Polyzoa; and Nitsche, Beiträge zur Kenntniss der Bryozoen.

The Bryozoa are divided into two orders: (1) the Phylactolæmata which are fresh water. The tentacular disk is horse-shoe shaped and the mouth has an epistome or cover which closes it. (2) the Gymnolæmata have the tentacular disk circular. These are marine, and as my studies have been confined to these I shall only consider them to-night.

The first suborder, the Cheilostomata represented by Flustra, Lepralia, Eschara, &c., is the largest and most important. They have an operculum to the aperture of the zooecium (cell) while the Cyclostomata have none. These last are all calcareous, and the form of the cell is more tubular than in the Cheilostomata, and on this character was based d'Orbigny's classification. The best known form is Crisia. The Cyclostomata have had much less attention than the other divisions.

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* The zooecium is the cell in which is the polypide, consisting of the alimentary canal and the tentacles. As the word cell is used for a single cell of cellular tissue and for a chamber (as the bee's cell) it is necessary to use the term zooecium for the sake of clearness.

† Elenchus Zoophytorum, p. 19.

The next suborder is the Ctenostomata which is distinguished by the cell orifice being surrounded by a fringe of bristles. These are horny or fleshy, and none are calcareous, and have a soft stalk with zooecia springing out from the side.

The Endoprocta (Pedicellina, &c.) consist of some half dozen species on the border land, and need not be further discussed in remarks of so general a character.

The form of the Bryozoa is very various, but all are fixed, and this, as we have seen, was so much associated with vegetable life that it was one of the reasons which led to the strange conceptions alluded to.

The marine forms live at various depths, occurring probably in greatest abundance in from 20—100 fathoms, but there are also many species living just below low water; and here, as with other marine animals, we can, from the species, judge of the depth. In depths where the currents are slight and which we may consider as below the Laminarian the firm forms as Eschara, Idmonea, Retepora, &c., abound, whereas in the zone, where they would be broken by the moving waters none, except incrusting forms, are rigid for any considerable extent, but some have chitinous joints at shorter or longer intervals as in Crisia, Cellaria, Salicornaria, &c., some have but very little calcareous matter so that they are flexible like most Flustra and the Ctenostomata, and even in Flustra membranacea the thin calcareous portion has been shown by Nitsche to be in discontinuous plates, thus giving a a large degree of flexibility to this semi-calcareous species. Although the rigid forms are not found in the lesser depths many of those of great flexibibility as species of Crisia Salicornaria, &c., &c., are found in the deeper waters. The Challenger found Bryozoa at all depths, and doubtless this expedition will add very much to our knowledge of their range about which but little is yet known.

In many the branches anastomose, thus forming a reticulated structure as in Retepora, Retihornera, and Fascicularia; and the Fenestellidae, which were so numerous in the Paleozoic seas, grew also in a reticulated form, and from this they have been considered to be allied to Retepora, but they entirely differ in every other respect from Retepora, and as I have just had the opportunity of observing in Actinosoma fenestratum, Young, the so-called denticles are so thin and reach so nearly to the centre as to appear like distinct surface septae and seem to indicate that the Fenestellidae should not be placed with the Bryozoa.

We said that the Cheilostomata were sometimes calcareous and sometimes horny, and as the Flustra (sea-wrack) and Eschara are the best known to casual observers we may describe the polypides in these and say that they only vary from others in minor details. The polypide is provided with tentacles (varying from 8 to 30) which are covered with cilia, and from this the Bryozoa were called by Farre* the ciliobrachiata. The polypide, including the tentacles, when withdrawn, lies in a transparent sheath, which is held in its place by sets of muscles from the cell wall, and besides these there are the large retractors attached near the base of the polypide; the opercular muscles, which in some species, as Myriozoon truncatum, attain great dimensions; and in the non-rigid species the parietal muscles.

The muscles, instead of occurring in solid bundles as they do in the higher animals, are in separate threads. Although not entering into any details concerning the muscular system, it is not because their position is not of considerable importance when considering the zoological affinities of the various Bryozoa and other lower forms of life; and in advocating their relationship with the worms, Schneider† has pointed out that in the worms their specific and generic place can be determined by means of their muscular system, in the same way as that of the vertebrates by their bones.

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* "Observations on the minute structure of some of the higher forms of Polypi." Phil. Trans., 1837, pt. 2.

† Schneider. "Archiv. für Micro. Anat.," tom V., 1869.

The reproduction and increase takes place by means of budding and of eggs. The egg developes into a ciliated larva, which undergoes a change that has been compared by Joliet* to that taking place in the developed polypide, so that the young polypide buds in the larva, which thus represents the zooecium. This zooecium now divides into two in somewhat different ways in various species, and the growth of the colony proceeds by division of the growing zoœcia. We may, however, examine a little further as representative, the growth of a Flustra or Eschara, as worked out by Nitsche in Flustra membranacea. The cells or zooecia become long, almost tubular, and after the polypide has begun to bud in the lower part a division takes place across from the lateral walls, thus dividing the lower part off into a rectangular cell the usual shape and size. In this way the Flustra increases in length, and the increase in breadth is brought about by the division of these terminal zooecia in the direction of their longer axes.

The budding of the polypide inside the cell has been followed out by several observers, as Nitsche, Claparède, Smitt, &c.; and a reference to their figures will explain more than pages of description. The bud, which at first is only a cellular protuberance, soon shows divisions which represent the tentacles, and ultimately these become separated tentacles, the canal of the polypide becomes fully developed, the oral aperture is now formed in the zooecium, and the now completed polypide can extrude itself through this opening, and the cilia on the tentacles create currents which bring a supply of food to the mouth of the polypide. After a time, it shows symptoms of loss of vitality, and then in a very short time the polypide has passed through a gradual diminution until it has disappeared, leaving only a small, dark body, enclosed in a thin membrane, and the zooecium is now empty, excepting the parietal muscle, the funiculus, and the "dark body." Joliet says, in his recent paper, that he has found this process of decay and absorption to take place in one case in ten hours. As we have before said the zooecium does not remain empty but another polypide may bud inside the same zooecium and frequently traces may be seen showing that there have been two, three, or even occasionally more.

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* "Bryozaires des cotes de France," par M. Lucien Joliet. Lacaze-Duthiers, Archives de Zoologie Experimentale, 1877, pt. 2.

The "dark body" just mentioned is the remains of the polypide, and as recent observations seem clearly to prove has no vitality, though it has been maintained by Smitt and Hincks that the new polypide sometimes buds from this; and by early observers this was thought to be an ovum. When the polypide should happen to bud near this "dark body" it may become enclosed in the new bud, and in this case will, as watched by Joliet, be passed out through the intestine with the first undigested food.

The funiculus mentioned also requires further explanation. The funiculus is well known as a flexible cord at the base of the polypide in the fresh water Bryozoa, but the homologous organ in the Cheilostomata has much larger extension in the zooecium though not as prominent. The polypide is here also attached at the base by the funiculus, and it is from this that the budding of the polypide takes place, and in the Cheilostomata the funiculus of the one cell has a connection with those of the neighbouring cells. It is thus quite clear that what is called the funiculus is of primary importance. This term has been used by many recent writers on the Bryozoa and *Nitsche calls the lateral funiculus the seitenstränge, or "funiculi laterales," which he says does not prejudice the physiological signification, and in the same sense the term is used in this communication.

This filament is composed of fusiform cells, and this is covered with the corpuscles of the chylaqueous fluid. From the principal filaments branches are given off. This is what

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* "Beiträge zur Kenntniss der Bryozoen." Zeitsch für wissench. Zool., 1871.

was called by †Reid the contractile substance; by ‡Müller colonialnervensystem; by §Reichert the colonialbewegungsorgan; and within the last few months ||Joliet has suggested the term cœnosare; but I think he may have overlooked the fact that Hincks and others have already used this term in a somewhat different sense in the Hydrozoa, though as the term funiculus is so far from satisfactory now that more is known of the funicular system, it is quite possible that if this stands criticism, and Joliet is satisfied to retain it, that it may be used instead of the older term which has lost much of its original significance.

Reid mentions change of this contractile substance in the avicularia; Reichert found a slow change take place in his colonialbewegungsorgan which he did not perhaps interpret quite correctly, and Joliet has shown that the finer anastomosing threads of the funiculus or coenosarc often change their positions altogether in the course of one or two hours. This is of course a very strong argument against this being a nervous system, for we see that these threads occur in various positions in the different cells of the colony, and in each the arrangement varies from time to time, which has no parallel in any nervous tissue.

Joliet believes the greater extension of the funiculus in the marine Bryozoa is represented by the middle layer of the endocyst of the fresh water, but as I am not practically acquainted with this group I cannot express any opinion.

In the fresh-water Bryozoa a nervous ganglion has long been known, but with the exception of Pedicellina none is known with certainty in the Gymnolæmeta, though in a few instances it has been thought that one could be distinguished.

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† "Anatomical and Physiological observations on some Zoophytes," by Jno. Reid, M.D., Ann. Nat. Hist., Vol. XVI., 1845.

‡ "Das Kolonialnervensystem der Moosthiere." Wiegmann's Archiv, XXVI., 1860.

§ Abhandl. der König Akad. der Wissensch. Berlin, 1869.

|| Loc cit.

As the only part which geologists are likely to find, except in special circumstances, is the calcareous shell, this has for me the most interest, but as I have recently entered into the points which I more fully examined, it will not be necessary to do more than briefly review a few points concerning the shell.

Through the shell of most there are tubes passing from the interior to the exterior, and these tubes are filled with the chylaqueous fluid of the animal, and upon these perforations or pores depends for the most part the sculpturing of the shell. There is an external growth which is most apparent in Eschara, Myriozoon, &c., and this takes place independently of the life of the polypide, as is shown by a considerable external growth over the opercula.

The stalk of the Ctenostomata is divided by diaphragms in which there are minute perforations through which the funiculus above and below is connected, and in the same way each polypide is joined with the main funiculus. In the walls of the Cheilostomata there are similar thin portions, which have been called by Reichert "rosettenplatte," and the object of my recent communication, to which I have referred, was to show that the position and form of these rosettenplatte should be given as a character, since they differ very much in various species. The points to be noticed are their number, position, and if they are separated by dividing ridges. Part of the characters it will be seen can be made out in fossil species, and therefore it is most important that it should be mentioned, as thus the position of an internal organisation is recorded in the shell.

Although this is an important point that may be learnt by an examination of thin sections of the shell, it is by no means all, for sections will show the shape of the cell, mode of growth, varying structure of the shell, from which in some cases even the general sculpturing of the exterior would be revealed. Nor is it probably with the more recent formations only that sections will aid in our knowledge of the soft portions. I have as yet had little opportunity of examining Bryozoa older than the chalk, but in some Scotch Carboniferous shale I had given me there are some so well preserved that much of the structure of the shell can probably be worked out besides the shape of the cell; and the pyritic crystals between the cell walls show where there was organic tissue so many million years ago.

I cannot help thinking that if more attention was given to sections of the small fossils, then we should be able to fix the age of most limestones from *minute* fragments, and should be able to know with much more exactness what animals lived in the seas of past ages; but it is necessary that recent forms should first be examined to give a basis of comparison. Enough has been said about these animals to enable the next point to be now understood.

It may be a matter of surprise that I should speak of the Bryozoa while the two first authorities, Dr. Busk and Dr. Allman, employ the name Polyzoa for this class, and I therefore think it necessary to state my reasons, and leave each one to consider whether they are sufficient or not.

My principal reason is, that after a struggle of nearly half a century, the Polyzoists are in a great minority, for in all the countries of Europe, with the exception of England, the term Bryozoa is used, and has been adopted by d'Orbigny, Hagenow, Bronn, Van Beneden, Krohn, Haime, Leuchart, Reichert, Reuss, Nitsche, Kirchenpauer, Smitt, Roemer, Claperède, Manzoni, Joliet, and many other zoologists who have not made them a speciality. Sars, in a short paper, uses Polyzoa, and Leidy and Hyatt, in America, have followed their English leader, while Dana is a Bryozoist.

As uniformity of nomenclature is in science of such great importance, it is much to be wished that one term should be used by all. However, if there were not other reasons, it would seem right for science to be encumbered by the

British name as well as the more general one. Yet those who have called these animals Bryozoa have not done so without a reason nor without fully recognising the fact that Thompson has priority over Ehrenberg. Bronn, as far as I am aware, had not devoted special attention to this class, but as he was so justly ranked, among a generation now passing away, as the greatest authority on classification, his opinion carries much weight, though he may have received assistance in the details. He points out that the term Polyzoa, used in the separation of animals from others which this name would as correctly describe, is very unphilosophical and could not be accepted. The term Bryozoa may be thought open to the same objection, though as here only an external appearance is taken and nothing approaching a zoological character, the objection does not apply in the same way.

But the great objection of those who have refused to adopt Polyzoa is the entirely different meaning* Thompson attached to the word to that which it now conveys. The heading of Thompson's paper is most important, and runs, "On Polyzoa, *a new animal* discovered as an inhabitant of some zoophytes," and in the text he says, speaking of Flustra, "in many of which I have clearly ascertained the *animals* to be *Polyzoæ*" (italics mine). Dr. Busk says that etymologically the name as now used is not the same as when originally given. I am unable to quite agree with Dr. Busk in this way of putting it, for the difference seems to arise from an entirely distinct zoological conception, and "the inhabitant of the zoophyte" is the polypide in the zooecium, or cell, and the Polyzoæ are the polypides, so that by the term Polyzoa nothing more was then meant than a single polypide. The intimate connection of polypide and zooecium seems to be ignored as much as if we were being taken back to the time of Pallas. Our knowledge of an intercolonial connection shows how unfortunate was the conception of Thompson. If it was not for conflicting claims the due meed of praise could be given to Dr. Thompson for such good investigation in this and several other branches of zoology, without pointing out that the general idea is not in accordance with the present knowledge of the subject.

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* "Zoological Researches," Mem. 5.

As far as I am acquainted with the subject, it appears that all the fossil Bryozoa, with the exception of the tertiaries, have been too superficially described for any palæontological generalisations to have any value, for until much careful examination has been made, we shall feel uncertain as to which are Cheilostomata and which Cyclostomata, and the fact of Fenestella, which occur in such great numbers in the Paleozoic rocks, possibly not belonging to the Bryozoa, shows how much there is still to be done in micro-palæontology.

One point which adds much to the interest of the Bryozoa is their wide relationships. As mentioned, they have long been removed from the Anthozoa. Milne-Edwards, Dr. Allman, and others have shown that there is an affinity with that interesting class, the Ascidians, the great interest of which consists in their connection of widely separated classes. Then with the Brachiopoda the connection is undoubted, and Schneider has shown that the embryonic development most nearly resembles that of the Gephyrea, an order of worms. Schneider also proved that Cyphonautes was the larva of Bryozoa, so that here, as in many other cases, the young form was previously thought to be an animal with an altogether different zoological position. As embryology is such an important factor in assisting classification, the Bryozoa are now considered to be most nearly related to the worms through the Gephyrea, and this has also removed the Brachiopoda towards the worms, so that instead of being most intimately related to the Gasteropoda and Lamellibranchiata they are less closely connected.

The question of individuality it will be seen is, in such a class, one of great difficulty, and if we formulate it that may give us some satisfaction, but it may, after all, be a question which we do not understand any better in consequence. The recent investigations, which show that a much greater importance must be attached to the funiculus than was supposed, somewhat alter the basis upon which this question has been previously considered.

It has been seen that the difference of the growth of the polypide and zooecium was felt to be so great as to place the zoophytes in a kingdom intermediate between the animal and vegetable kingdoms, and the general conception, as far as it concerns this class in showing the independent growth of the polypide and the cell containing it, was correct as far as they do not necessarily grow simultaneously.

Lamark* thought the polypides were quite distinct and had no more connection with one another, nor any more with the cell than the wasp has with its cell. This is, perhaps, the most simple theory of any concerning their individuality, but at the same time apparently the least correct.

Grant, and other observers afterwards up to the time of Dr. Allman, looked upon an individual as made up of zooecium and polypide. Dr. Allman, however, considered that besides the zooecium and polypide, the ovarium and testes each represented a zooid and the embryo also represented one. This, in a more or less modified form, has been generally accepted, but the recent researches in the marine Bryozoa seem to require, at any rate, that the ovaria and testes should not be looked upon as separate from the polypide. Schweigger,* on the other hand, thought the Bryozoa colony represented an individuum with a number of mouths and stomachs; and Claparède has since then maintained that the polypides should be considered as organs of the Bryozoa.

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* See Nitsche, loc. cit., p. 59.

In whatever way we may express ourselves the nature of my recent investigations has impressed upon me the fact of a certain connection of the polypides, and we have seen that the polypide is not necessarily a descendant of the zooecium, as it may grow from the funiculus. I further think that the zooecium may grow from the same organ. In Cellaria one or more chitinous tubes are thrown out from the oral aperture, and through this the funiculus passes, and I believe that it is from this, and not from the chitinous tube, that the zooecium and then the polypide is formed. This I can only say with great reserve until I have the opportunity of watching the growth of this or a similar genus; or I may perhaps better say that I point this out as a line of investigation for anyone at the sea-side.

The early stages of such organs as the avicularia radical fibres, ovicells are the same as the early stage of the zooecium, so that the "cystide" (Nitsche) of each is, according to all theories which recognise the zooecium as an individuum, to be looked upon as of the same value, each being also an individuum.

Perhaps in every scientific subject the man who has entered into it feels how much there still remains to be done, and certainly the present could be no exception, as there are in this field of research many parts of virgin soil requiring the co-operation of many workers.

Mr. Plant, F.G.S., exhibited specimens of auriferous quartz from Australia and North Wales, showing on their surfaces certain changes and metallic growths, which had taken place within the last ten years. He also referred to a paper recently read by Mr. T. A. Readwin, F.G.S., at the Mineralogical Society "On Mineral Growth at ordinary temratures and under ordinary circumstances."

The object of its author was mainly to prove that with specimens in cabinets and in cases he had observed a spontaneous metallic-growth chiefly upon the surfaces of rock specimens from mines and upon ores of some of the metals. The paper described in details many such, from the author's cabinet, and from the museums in England and on the continent, chiefly growths of gold, silver, electrum, or argentiferous gold, copper and titanium.

The specimens exhibited by Mr. Plant had been identified by Mr. Readwin as gold-growths in thin sheets, and as globules of gold, and Mr. Plant was able to authenticate the development or growth of the gold upon the specimens since 1868. He had specimens of copper also recently developed as stalactites or moss copper. Changes in pyrites and sulphides generally were a well-known nuisance in all collections, but these metallic-growths were a new thing to him, and it is yet an interesting point to learn their cause, and watch whether the end be amorphous bundles or true crystals.

The paper, of which the following is an abstract, was read at a meeting of the Section, held January 14th, 1878:—

"The Mollusca of Cymmeran Bay," Part IV., by JOHN PLANT, F.G.S.

For four years a systematic search has been made in this locality for the local mollusca, and already the Section has had several communications upon the species, lists of which have appeared. The additions in 1877 were very few, as the summer was unfavourable for much dredging outside the bay, but the total of species found on shore or dredged is now two hundred.

The bay is five miles wide from Rhoscolyn Beacon to Ynys Mabion, with a broken shore line of above 9 miles, part bold cliff, part sands, and some reef—the bay is full of reef rocks running NNE by SSW., with narrow vallies of sand between—averaging 9 fathoms deep at high water—the reef ends about two miles from the shore, and abruptly, as the water outside deepens from 14 to 20 fathoms. This reef is a barrier cliff which causes a great roll or swell in the water when the tide is coming on strongly. No dredging is possible within the bay because of the reef, and all the shells dredged are got outside on shingly, sandy or rough bottoms in from 15 to 30 fathoms. The oyster beds lie out here. No loose valves of shells would ever get into the bay by bottom drifting from distant places, as the submerged cliff of the reef would stop them outside. The bay is too cold for luxurious growth of sea weed, although after storms the shore is strewn with stalks and leaves of the larger algæ. Yet the rocks at low tide are not covered with much growth,—the sea and storms are too rough and the shore often changes its aspect from sand or shingle drifted here or there. The tide rises 24 feet at its highest on the coast. The fauna is more northern than southern in its character—the proportions out of 100 dredged shells are, Arctic and Boreal species 28, Celtic and Southern 14, leaving 58 species as common to the British Coasts.

The scarcity or abundance of some of the dredged species are interesting points to record. Saxicava rugosa, Venus striatula, Cyprina, Phasianella, Trochus ziziphinus, Nassa and Nucula are plentiful; Astarte sulcata, Velutina, Lamellaria, Lucina borealis, Leda caudata, Fusus antiquus, Trophon muricatus and clathratus, Trochus tumidus, T. Montagui, Venus casina, V. verrucosa, V. fasciata, V. ovata, Pileopsis, Otina, and others were sparingly found. The new species are Lima Loscombii, Circe minimua, from 18 fathoms, and several Odostomia and Rissoa. The young Cypræa dredged were empty, and none of these dredged shells were so fresh or of so good a colour as others found on the shore.

Ordinary Meeting, April 2nd, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

The PRESIDENT said that he had come into possession of three interesting letters from the late Sir W. Fairbairn, Bart., F.R.S., a former President of the Society, to the late Professor Hodgkinson, F.R.S., also a former President. As both parties have left this world there can be no harm in giving the letters to the public. My friend Professor Hodgkinson, as is well known to the older members of this Society, devoted his whole life to science for its own sake, and gave nearly fifty years' labour to the world without fee or reward. His formulæ for the construction of cast iron beams and pillars are known and used wherever that metal is employed; and this is the only reward he received, either at home or abroad, with the exception of a couple of medals and a marble bust in this hall. The letters speak for themselves, and are as follows:

Rotterdam, Aug. 6th, 1836.

My dear Sir,

I have no doubt you have already deemed me a lame assistant in preparing the report for the approaching meeting of the British Association, a mere sleeping partner: ready to share honours due to your exclusive labours. I have tried in vain to make something of the experiments you were kind enough to hand to me in London. I have examined them with great care, but unless I had all the facts and subsequent experiments before me I find it would be worse than useless to attempt, or rather spoil, a much more accurate and satisfactory description from Mr. Hodgkinson. The fact is, I must confine my enquiries to an examination of the metals experimented upon; investigate the mixtures and make such experiments during the winter as will lead to pretty correct results tending to show the application of the different metals and their combinations for the purposes of the arts. I think this will prove an important enquiry, exceedingly useful in its application, and not without interest as an appendage to your more elaborate investigation on the strength of the metals.

I made several attempts at an introductory paragraph for the forthcoming report; but as it was with the experiments, so it was with the introduction. I was short of the correspondence, and had to abandon an almost hopeless case for want of material to go on with. If I can assist you on my return this day week, I shall have great pleasure in doing so, particularly as we shall have 8 or 10 days before the meeting.

I have given up all hopes of being able to attend at Bristol. I trust you will go and be present at the meeting, and I hope your sister's health will permit of your absence for that period of time. I am already a week behind time. I must apply myself to business immediately on my return, and abandon all thoughts of absence for some time to come. Anticipating the pleasure of seeing you in a few days,

I am, my dear Sir, in haste,
Ever faithfully yours,
W. Fairbairn.

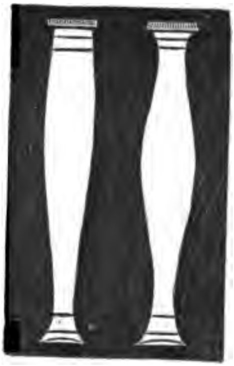
To Eaton Hodgkinson, Esq.,
Windsor Bridge,
Manchester.

London, April 24th, 1838.

Eaton Hodgkinson, Esq.

My dear Sir,

I was so pestered for time and had so much to do before leaving Manchester that I left some matters on which I intended to consult you untouched. I am glad you are making such good progress with the pillar experiments. Now that you are fairly embarked, I am desirous you should see them to a conclusion upon terms that ensure satisfaction. I think a great deal may be done and much light thrown upon the subject by a careful and well conducted class of experiments; in hollow tubes there is great scope, particularly as relates to varied diameters, thickness of metal, and the proportions of the upper to the lower diameters. Also an investigation into the proportions of the ancients, in order to ascertain whether the small rising from the lower base of the shaft is intended for beauty or strength: probably both qualities are included, and may be the result of experiment. In point of form you are aware that the symmetry varies in the different ages of the Egyptians, Greeks, and Romans, at any rate the swell observable in the best proportioned columns of the antique are valuable as far as it prevents deflection and must add to the strength of the shaft. I speak from memory as regards the proportions, but I think it would be useful as well as interesting to ascertain the strongest form, and probably the diameter near the middle will be the nearest approach. The antique is of this form:—but you can easily ascertain these particulars in any work on architecture.



Having completed the experiment on columns, the next business will be to prepare the papers for the Association. I have written a few pages introductory to the experiments on the transverse strains, but I think it will want remodelling, as well as considerable additions.

When I have the pleasure of your presence here in London it will be a good opportunity to get the papers printed. We could then read over the proofs and superintend the press at the same time. What think you of this? Will you write to Phillips and arrange it accordingly? We shall never have a more fitting opportunity.

Although it would afford me gratification to have my name associated with so able an experimenter as yourself, yet I cannot consent to have it attached to any document to which I am not a contributor. You are sensible I am no mathematician, and on that account cannot afford to lose a single fraction of the small offerings I have made at the shrine of science. It is different with my friend Hodgkinson, whose colossal dimensions might stand paring and is not so easily reduced. The fact is, I must be permitted to rest upon such humble merits as I possess, and not in a character which the world might justly call assumed. If you think my name can be respectably and justly introduced as a coadjutor in the more simple forms of transverse strain, I am satisfied: if otherwise, I will then take the experiments on temperature, continued strains, &c., and leave all the other to a mind of much greater profundity than my own.

London, Aug. 17th, 1843.

My dear Sir,

(After alluding to a domestic affliction.) Under these afflicting circumstances of course you will not expect me at Cork. I am however with you in mind, and would have been much gratified to have spent not only the days of the meeting with you, but to have accompanied you through some of the most interesting scenes in Ireland. This however cannot be accomplished, and all that I can wish you is an agreeable and satisfactory meeting with your scientific and learned friends.

I have written both Mr. Phillips and Mr. Taylor, requesting they will renew the committee with the grant of £150 for enquiring into the changes produced in metallic bodies by concussion. You will have the goodness to support the application; and having collected all the broken axles on the Leeds railway and the drawings of a considerable number on the South Western, I do hope with your assistance to make up a report by the time of the next meeting.

In requesting your assistance I must confess I have some little misgivings. I think, but I may be wrong, that I have observed some faint appearance of an ugly monster which they call ——— which sometimes darkens the amiable and bright side of your character. You must however dispose of these impressions and deal honestly with me in all your thoughts. I can assure you you have nothing to fear from the quarter you seem to apprehend danger. I have neither the time nor the talent to trench upon your investigations as a mathematician and an experimentalist. I rejoice in your success in anything. I am never jealous of your power, and why deny to your friend and supporter a small share in the merits of a reputation which on many occasions he has endeavoured to extend and by every means in his power to perpetuate? You must never look upon me as your competitor, but as your friend and fellow laborer in a totally different department of science. The time is going, and begging you will receive the assurance of my attached friendship and regard,

I am yours affectionately,
W. Fairbairn.

Eaton Hodgkinson, Esq.,
British Association,
Cork.

P.S.—I have no time to reexamine what I have written, and must give it you with all its imperfections.

Rider.

I feel grateful for the close and continued attention you pay to the lads. In my hurried and anxious life I derive much consolation in the thought that my sons have so able and so willing a preceptor at hand; I can assure you I am not insensible to the advantages they must attain from your unwearied attentions. I live in hope to see them do credit to their instructor; and on some future occasion to prove themselves no despicable mathematicians. At present you have no sons of your own, and until the time arrives when some sturdy brat shall scream Papa I must view you in the light I have always done, as a second father to my children. I sat down to write you a few lines about the pillars, it has turned out a long letter, which altho' of some slight interest to myself is nevertheless unworthy of postage. I must therefore look out for a frank, and remain

Ever faithfully yours,
W. FAIRBAIRN.

I have a scheme in embryo, which I think I shall ruminate on, namely, an account or disquisition upon our iron ship building, accompanied with a few readable experiments for the Association. I will want you to put a correcting hand to the document in order to prevent it turning out the mountain in labour.

"On Aurin," by R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

In a paper read before this Society, on 31st October, 1871, we gave a short account of this colouring matter, which was discovered by Kolbe and Schmitt in 1861. We showed that the commercial product, which is called aurin or corallin, and is obtained by heating of phenol with sulphuric acid and oxalic acid, is a mixture of different bodies from which we isolated a colouring matter which crystallised exceedingly well and for which we retained the name of Aurin.* The analysis of this body gave numbers agreeing with the formula $C_{20}H_{14}O_3$, from which we concluded that it was produced by the action of nascent carbonic oxide on phenol:

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* A full account of this investigation is found in Journ. Chem. Soc. (21, XI., 434.)

$$3C_6H_6O + 2CO = C_{20}H_{14}O_3 + 2H_2O.$$

We further showed that aurin, when treated in an alkaline or acid solution with zinc-dust, combines, like most other organic colouring matters, with hydroden, yielding leucaurin $C_{20}H_{16}O_3$, which readily crystallises in large colourless prisms, whilst on heating aurin with ammonia under pressure a new red colouring matter is formed, which is called red aurin or peonin.

The further investigation of this body showed that it contained nitrogen, which on heating with soda-lime was evolved in some cases completely as ammonia, while in others, where the action of the ammonia went on longer it was given off, at least partly, in the form of aniline. We then found, that on continuing the heating, the red colour of the liquid became paler, and at last a yellowish solution was obtained, from which water precipitated a white crystalline body, possessing all the characteristic properties of rosaniline.* The formation of this compound might be explained by the following equation:

$$C_{20}H_{14}O_3 + 3NH_3 = C_{20}H_{17}N_3 + 3H_2O.$$

But Hofmann, who has most carefully examined rosaniline as well as a great number of its derivatives, found that the formula of this base is $C_{20}H_{19}O_3$, and this result has been confirmed by Caro and Gräbe,† who showed that rosolic acid, which is obtained from rosaniline by a reaction, which shortly may be stated to be the inverse of that above given, has the composition $C_{20}H_{16}O_3$, while its properties are in almost all respects so similar to aurin, that some chemists, including ourselves, have been inclined to regard them as identical.

In order to clear up these points it was necessary to prepare a considerable quantity of pure aurin. We have already shown in our memoir read before the Chemical Society,‡ that a pure compound is readily obtained by heating pure phenol with oxalic and sulphuric acids for a few days to about 105°C. Although a good product is thus obtained, the yield is but small, while on heating the mixture longer or to a higher temperature bye-products are formed. Some chemists have stated that the formation of the latter cannot be avoided, and that it is very difficult to obtain thus a pure aurin. We believe that we are able to explain these different results; the formation of a pure compound depends much on the proportions and the manner in which phenol and the acids are mixed. As however the preparation of pure aurin by this method is a very tedious process, we endeavoured to obtain it from the commercial product by the method which we have described in our first paper and purifying it by repeated recrystallisation from alcohol, the crystals separating out first being used for further purification, because we found that the purer aurin is, the less soluble it becomes, its solubility being increased by the admixtures which are present in the crude product.

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* Journ. Chem. Soc. (2)XI, 434. † Liebig's Ann. CLXXIX, 148.

‡ Journ. Chem. Soc., 1877, II., 121.

Another method for purification which we employed consisted in washing good commercial aurin with cold alcohol until only a comparatively small residue was left, which was then further treated as above stated.

The different crops of crystals were analysed, and the purification continued until the product did not change its composition any more. A great number of analyses were thus required, and we have great pleasure in expressing our best thanks to Mr. L. T. O'Shea, who performed them with great care.

The results did not, however, agree with our former; the product having a constant composition, and crystallising exceedingly well, gave numbers agreeing with the formula $C_{19}H_{14}O_3$. We therefore again prepared aurin from pure phenol, and crystallised it repeatedly from alcohol, until its composition became constant, and found it to have the same formula.

Aurin therefore appears to be the lower homologue of rosolic acid; like the latter, it readily forms a tetrabromo-compound, which we obtained pure by the following process. Pure aurin was dissolved in hot glacial acetic acid, and this solution poured into a mixture of an excess of bromine and acetic acid. The liquid remained perfectly clear, but on standing for a few days the tetrabromaurin crystallised out almost completely. It forms small brownish green crystals, with a metallic lustre, which dissolve in alkalis with a purple colour. The determination of the bromine gave numbers agreeing closely with those required for the formula $C_{19}H_{10}Br_4O_3$.

Whilst this investigation was going on a new light was thrown on the subject by the researches of E. and O. Fischer. In order to elucidate the constitution of rosaniline these chemists endeavoured to convert rosaniline into the hydrocarbon from which it is derived. In this they did not succeed, but they were able to transform leucaniline $C_{20}H_{21}N_3$, which stands to rosaniline in the same relation as leucaurin to aurin, into a hydrocarbon $C_{20}H_{18}$, which is the mother substance of this base, and from which it follows that the hydrocarbon corresponding to rosaniline has the composition $C_{20}H_{16}$, which also confirms the correctness of Hofmann's formula for rosaniline. The leucaniline, which they employed was obtained from commercial rosaniline; but Rosenstiehl has shown that this body, which is prepared by oxidising a mixture of aniline, solid paratoluidine and liquid pseudotoluidine, contains several, probably isomeric, bases. E. and O. Fischer therefore in continuing their research* prepared one of them in the pure state by using a mixture of pure aniline and pure paratoluidine. The product was converted into the leuco-compound, from which, to their surprise, they obtained, not as before, the hydrocarbon $C_{20}H_{18}$, but one having the composition $C_{19}H_{16}$ or the lower homologue of the former. They found the new hydrocarbon to be identical with one already known, which was discovered by Kekulé and Franchimont, and called Triphenylmethane $CH(C_6H_5)_3$ as it is marshgas or methane, in which three atoms of hydrogen are replaced by three phenyl-groups. Hemilian, who afterwards obtained the same hydrocarbon by a different method, examined it more fully and found amongst other results that concentrated nitric acid converts it into the trinitro-compound $C_{19}H_{13}(NO_2)_3$; E. and O. Fischer prepared the same body, which by the action of nascent hydrogen was converted into a base having all the characteristic properties of leucaniline. This was heated with arsenic acid, in order to abstract hydrogen by oxidation, and the rosaniline which formed the starting point of their last investigation was obtained again.

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* Ber. Deutsch. Chem. Ges., XI. 195.

From these results it follows that commercial rosaniline consists principally of the base $C_{20}H_{19}N_3$, and is probably a derivative of pseudotoluidine, while the rosaniline from paratoluidine and which has therefore been called pararosaniline is its lower homologue, having the formula $C_{19}H_{17}N_3$. The rosaniline which we obtained from aurin is undoubtedly identical with the latter,* its formation being readily explained by the following equation:—

$$C_{19}H_{14}O_3 + 3NH_3 = C_{19}H_{17}N_3 + 3H_2O.$$

The question which now has to be solved is how to explain the formation of aurin; we have already made experiments in this direction and obtained interesting results, which, however, are not yet sufficiently worked out to publish them.

Another question to be elucidated is, what is the chemical constitution of aurin? According to E. and O. Fischer that of pararosaniline is $C_{14}H_{11}(NH_2)$, from which it follows that aurin is $C_{14}H_{11}(OH)_3$. Already, in our first research, we obtained results which seem to confirm this view. On heating aurin with acetyl chloride, we obtained a colourless crystalline compound, which on analysis gave results nearly agreeing with the composition of triacetyl aurin $C_{19}H_{11}(OC_2H_3O)_3$, and on using benzoyl chloride a similar benzoyl-compound was obtained. The analytical numbers did not, however, sufficiently agree with those required by theory, and therefore we did not publish them; and, as the purification of these compounds was found to be beset with several difficulties, we did not then examine them further, because we believed the study of red aurin would more readily throw light on the subject. We have now prepared them again from pure aurin, and hope to be soon able to settle these points.

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* Since this was written E. and O. Fischer prepared some pure aurin according to our directions, and converted it into a leucorosaniline, from which they also obtained triphenylmethane (Deutsch. Chem. Ges. Ber. XI., 473).

"The Origin of some Ores of Copper." Part II. By Charles A. Burghardt, Ph.D., The Owens College.

Atacamite.

This interesting and beautiful mineral occurs generally in radiating foliated masses, or in bundles of acicular crystals which are characterised by a fine vertical striation upon their vertical faces. Globular, reniform, and stalactitic atacamite is also known. Thus it is safe to infer that in a great majority of cases the mineral has been formed in some way from a solution of copper salt, and not by igneous action. It is also a well known fact that atacamite crusts are occasionally found resting upon clefts in the lava of some volcanoes, being formed by the action of hydrochloric acid gas evolved by the volcano upon copper, or a compound of that metal, but these crusts are insignificant in quantity and comparatively scarce. In Chili, Bolivia, and the West Coast of Africa, very large quantities of atacamite are obtained as a commercial product; it therefore becomes interesting to the mineralogist to ascertain under what conditions these formations arose, and in order to do this a thorough chemical examination of the mineral itself is necessary. Atacamite is known to occur in nature in three states of hydration, and as an example of the first I may mention a specimen from Algodon Bay, Bolivia, analysed by Bibra (Jahresbericht, 1858, 740), which had the following composition, viz.:—

Cu	59·25	per cent.
Cl	16·11	„
O	12·51	„
H_2O	12·13	„
	100·00	

The formula corresponding with the above analysis being $Cu_4O_3Cl_2 + 3H_2O$. The atacamite of Copiapo, Chili, as analysed by Field (Journ. Chem. Soc. (7) 3, 193) has the following composition, viz.:—

Cu	56·38	per cent.
Cl	14·95	„
O	10·78	„
H_2O	17·89	„
	100·00	

The formula for this being $Cu_8O_6Cl_4 + 9H_2O$, or $(Cu_4O_3Cl_2)_2 + 9H_2O$. The third state of hydration is represented in the atacamite from Botallack, Cornwall, analysed by Church (Journ. Chem. Soc. (2) 3, 212), and that from Tocopilla, near Cobija, Bolivia, analysed by Berthier (Ann.. des Mines (3) 7, 542) the composition being as follows:—

Botallack Atacamite.		Tocopilla Atacamite.	
Cu	52·90	Cu	53·26
Cl	14·76	Cl	14·92
O	10·49	O	9·37
H_2O	22·45	H_2O	22·45
	100·00		100·00

and the formula $Cu_4O_3Cl_2 + 6H_2O$.

Having given a general idea of the composition of the various atacamites, I will now point out how some of them can be prepared artificially. Field (Phil. Mag. (4) XXIV., 1862, 124) prepared the last-mentioned variety by adding a solution of calcium hypochlorite to an excess of cupric sulphate, and boiling the mixture for some time, when an apple-green precipitate was observed, which proved on analysis to be identical with Berthier's atacamite from Cobija. If the ebullition were not sufficiently kept up, the compound was found to contain only 4 atoms of water. Proust prepared a green compound (which no doubt was atacamite, although its exact composition is not given) by exposing moist cuprous chloride to the air, also by digesting a solution of cupric chloride with cupric oxide. In a former communication to this Society (Proc. Lit. and Phil. Soc. XVII., 1877–78, 27—36) I mentioned that on heating some crystals of cuprous chloride in water in a sealed tube at a temperature ranging from 160°—180° C, that minute green spots of a substance resembling atacamite were observed upon the sides of the tube. Further, on heating cuprous oxide and a strong solution of chloride of sodium together in a sealed tube, at 150°—180° chalcotrichite was formed, and also a green substance resembling atacamite. Wishing to ascertain whether atacamite could be prepared in larger quantity (in accordance with my supposition that all the copper ores are products of the decomposition of cuprous oxide, the latter substance being a secondary product of the oxidation of the metal itself) I made numerous experiments, mostly consisting in the heating of cuprous oxide with sodium chloride solution in sealed tubes, and obtained excellent results, by simply covering a quantity of cuprous oxide with a concentrated solution of sodium chloride, exposing this mixture to the air at the ordinary temperature, and stirring it up occasionally. By proceeding in this way a very concentrated solution of cuprous chloride in sodium chloride can be obtained. After standing a few days this solution begins to decompose, a green insoluble substance separating out continually, which can easily be filtered off from the liquid portion and well washed in cold distilled water, in order to free it from all traces of cuprous chloride and sodium chloride. The substance thus obtained was carefully dried over calcium chloride until its weight was constant, and portions weighed out and analysed quantitativly with the following result, viz.:—

Cu	56·25	per cent.
Cl	14·29	„
O	10·95	„
H_2O	18·51	„
	100·00	

It will readily be seen that the above analysis agrees very closely with the one made by Field already referred to; whence it follows that the substance prepared by myself is identical in composition with the atacamite of Copiapo, Chili. The rationale of the formation is briefly this—"the cuprous chloride held in solution by the sodium chloride takes up oxygen from the air very rapidly, giving rise to the formation of cupric oxychloride and cupric chloride (the latter soluble in water), both eventually becoming hydrated." The following equation expresses the first stage of the reaction, viz.:—

$$3Cu_2Cl_2 + O_2 = CuCl_2 3CuO + 2CuCl_2.$$

Nearly all specimens of atacamite are intimately associated with cuprite, so that it appears highly probable that the former mineral has been obtained from the latter in the manner stated, more especially as atacamite is found in veins of other copper ores, associated with quartz, chalcedony, &c., in diorite and syenite. Again, at Sierra do Lembe, near Ambriz, on the West Coast of Africa, there is a large occurrence of atacamite in the malachite deposits, the "gangue mineral" being quartz. Debray (Zeitsch. f. Chem. 1867, 276) prepared crystallised atacamite by heating together basic cupric nitrate and a concentrated sodium chloride solution for several hours at 200° C, also by heating copper ammonium sulphate with a strong sodium chloride solution at 100° C. Up to the present time I have not succeeded in preparing the mineral in a crystalline state. A curious fact I may mention, viz., that natural cupric oxychloride should increase in hydration by three atoms of water.

Ordinary Meeting, April 16th, 1878.

E. W. BINNEY, F.R.S., F.G.S., President, in the Chair.

"Note on the Occurrence of Dioptase on Chrysocolla, from Peru," by CHARLES A. BURGHARDT, Ph.D., the Owens College.

A short time ago, Mr. W. M. Hutchings, F.C.S., of Birkenhead, sent me some specimens of chrysocolla ($CuSiO_2 + 2H_2O$), from Peru, accompanied with a statement that there were some minute crystals in a cavity in one or two of the specimens which might possibly prove to be the rare mineral dioptase ($CuSiO_2 + H_2O$). I proceeded to make a crystallographical and chemical examination of the crystals, and found that although extremely small, the forms could be recognised under the microscope. The chrysocolla mass is eaten into in one or two spots, cavities being produced which are divided into numerous cells by the intersection of thin partitions of chrysocalla substance. The dioptase crystals occur particularly fine in small green tufts and sheaves attached to the partition walls of the cells, whilst those crystals clothing the interior of the cells are not so well developed as the others. The measurements so far obtained have not been satisfactory, owing to the extreme smallness of the crystals, but the forms observed are those peculiarly characteristic of dioptase, viz.: $\infty P2 - 2R$; the rhombohedron being extremely well defined. No other forms were observed, but a great many fine acicular subindividuals growing parallel with each other build up a large individual. Some of the crystals I carefully picked out and examined chemically with the following results, viz.: heated before the blowpipe they were infusible and turned brown, *not black*, probably owing to one of two causes, either (1) the flame was not a pure oxidising flame, and a little of the cupric oxide was reduced to cuprous oxide; or, (2) there was a slight admixture of quartz with the dioptase crystals. The presence of copper was proved by dissolving a crystal in a drop of hydrochloric acid, evaporating off the latter, redissolving the residue in a drop of acetic acid and adding potassium ferrocyanide, when the characteristic copper reaction was very marked. There was not the slightest effervescence on dissolving the mineral in hydrochloric acid, but flocculent particles of silica separated out. Mr. Hutchings obtained identical results in a chemical examination of the crystals. From the above tests, the crystallographical examination and the fine emerald-green colour of the crystals, there can scarcely be a doubt that they are really those of dioptase. Colourless quartz crystals were also observed associated here and there with the dioptase, but +R and —R were both present, generally nearly in equilibrium, and the prism faces exhibited strongly the characteristic horizontal striation, therefore coupling the marked differences in colour and crystal-form a confusion of the two minerals could scarcely be possible.

I believe this is the first instance observed of dioptase occurring simultaneously with chrysocolla, and according to my knowledge the first time it has been found in Peru, in fact out of Russia. The exact locality of the mine cannot unfortunately be ascertained. I am of opinion that the dioptase has been formed by the action of water upon the chrysocolla, the latter being a product of the decomposition of cuprite (which is always intimately associated with it) by a solution of silicic acid in water. Very fine botryoidal malachite sometimes occurs associated with chrysocolla and cuprite in the same locality in Peru. I hope shortly to obtain accurate measurements of the dioptase crsytals.

"On Indigo-blue from *Polygonum tinctorium* and other plants," by Edward Schunck, Ph.D., F.R.S.

The author, after referring to his investigation of *Isatis tinctoria*, the common woad plant, the results of which were communicated to the Society many years ago,* proceeded to give an account of some experiments he had recently made with *Polygonum tinctorium*, a plant employed by the Chinese for the manufacture of indigo, his object being to ascertain whether the colouring matter is contained in this plant in the same form as in the *Isatis*, viz. as a glucoside. His experiments led to the conclusion that the leaves of *P. tinctorium* contain a substance which cannot be distinguished from the indican of the woad plant. It is amorphous, soluble in water, alcohol, and ether, and by the action of acids is decomposed into indigo-blue and a substance giving the reaction of glucose, probably indigucine. When its watery solution is boiled or left to stand for some time, it undergoes a complete change, and then no longer yields indigo-blue by decomposition with acids, but indigo-red and other products, indican, as formerly shown, undergoing a similar metamorphosis under the same circumstances.

The author recommends for the preparation of this substance the following process :—The leaves of the plant having been carefully dried, are ground to powder and extracted with spirits of wine. The green alcoholic extract is evaporated at the ordinary temperature, a current of air being employed to assist evaporation. After evaporation of the alcohol there is left a brown watery liquid, which is filtered from the deposited chlorophyll and fatty matters, and mixed with acetate of lead solution. This gives a copious dirty yellow precipitate, which is filtered off. Basic lead acetate added to the filtrate produces a primrose-yellow precipitate, which is filtered off, washed with water, then with alcohol, and then suspended in absolute alcohol. On passing a current of carbonic acid gas through the liquid the precipitate is decomposed, yielding lead carbonate, while the liquid acquires a yellow colour. The filtrate is evaporated in a current of air, and water is added to the residue, which it dissolves for the most part. Sulphuretted hydrogen is passed through the filtrate to precipitate the lead in solution, and the liquid after filtration is again evaporated. The residue is treated with ether, which leaves a portion undissolved. The ethereal solution leaves on evaporation a yellow amorphous residue, having all the properties of indican.

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* Memoirs, 2nd ser., XII. p. 177, and XIV. p. 181.

The author made some experiments with the fresh leaves of the plant, from which he concludes that the leaf cells contain no ready-formed colouring matter with the exception of chlorophyll, and that the indigo-blue which is formed when the vitality of the cells is destroyed by extreme cold, organic lesion, or chemical reagents, is produced by the decomposition of indican, which commences as soon as the protecting influence of the living cell is removed.

It has long been known that some orchidaceous plants, such as *Bletia Tankervilliæ* and *Callanthe veratrifolia*, yield indigo-blue. The author examined the leaves of the former plant and obtained a solution, giving the reactions of indican, and he is consequently inclined to suppose that the latter will be found in all cases to be the source from which indigo-blue is derived.

The author mentions a fact which he thinks may be of interest not only to the chemist, but also to the physiologist. On one occasion an alcoholic extract of dried woad leaves, in which the indican had undergone partial decomposition by long standing, yielded on evaporation a quantity of a substance which, when purified, was found to have all the properties of tyrosine. Though it is possible that the tyrosine may have pre-existed in the plant, the author is inclined to think that, like the leucine previously discovered by him, it was a product of decomposition of indican under conditions of which he is at present ignorant, especially as some connection is supposed by chemists to exist between tyrosine and indigo-blue, and on the other hand tyrosine and leucine so frequently occur together as products of decomposition of protein compounds.

"On the Internal Cohesion of Liquids and the Suspension of a Column of Mercury to a height more than double that of the Barometer," by Prof. Osborne Reynolds, F.R.S.

Introduction.

The ease with which under ordinary circumstances the different portions of liquid may be separated is a fact of such general observation that the inability of liquids like water to offer any considerable resistance to rupture appears to have been tacitly accepted as an axiom. In no work on Hydrostatics does it appear that the possibility of water existing in a state of tension is so much as considered; and suction is always described as being solely attributable to the pressure of the atmosphere.

The limit of 32 feet, or thereabouts, to the height to which water can be raised by suction in the common pump and the sinking of the mercury in the barometer tube, leaving the Torricellian vacuum above, until the column is at most only 31 inches—sufficient to balance the highest pressure of the atmosphere—are phenomena so well known as to be almost household words with us. It is not, therefore, without some fear of encountering simple incredulity that I venture to state

The object of this Communication.

In the first place my purpose is to show that certain facts already fully established afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with the resistance offered by solid materials. In the second place I have to describe certain experimental results which as far as they go completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be suspended in a tube or water raised by suction. And in conclusion I shall endeavour to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.

The Separation of Liquid is not caused by Rupture.

Although the smallness of the force generally requisite to separate a mass of liquid into parts leads to the supposition that the parts of the liquid have but little coherence, it may be seen on close examination that this supposition is not altogether legitimate. For such separation of a liquid as we ordinarily observe takes place at the surface of the liquid—is caused by an indentation or running in of the surface, and not by an internal rupture or simultaneous separation over any considerable area. Thus, when we see a stream of liquid break up into drops, the drops separate gradually by the contraction of the necks joining them, as shown in fig. 1, and not suddenly as in fig. 2. And the ease with which portions of a liquid may be separated by the forcing or drawing in of the surface affords no ground for assuming that the liquid is without coherence, any more than does the ease with which we may cut a piece of string, cloth, or metal with sharp shears, or even tear some of these bodies by beginning at an edge, prove that they are without strength to resist great force when these are applied uniformily so as to call forth the resistance of all the parts of the body simultaneously. It is true that under certain circumstances we observe the

FIG. 1.

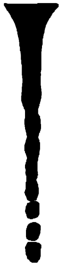
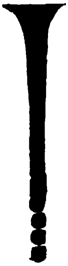


FIG. 2.



internal rupture of liquid—whenever bubbles are formed, as when water is boiled—but under these circumstances we have no means of estimating the forces which cause the internal rupture; they are molecular in their action, and for all we know they may be very considerable. Having thus pointed out that the ease of separation of the parts of a mass of liquid does not even imply a want of cohesion on the part of the liquid, I shall now point out that we have in common phenomena

Evidence of Considerable Cohesion.

These are for the most part what are considered minor phenomena; they are confined to the surface of the liquid, and are included under what is called "capillarity," or "surface-tension."

The phenomena of capillarity, or surface-tension, have recently attracted a great deal of attention, and many important facts concerning them have been clearly elucidated, some of which bear directly on my present subject.

Of the phenomena I may instance the suspension of drops of water, the rising of water up small tubes, the tendency of bubbles to contract, and the spherical form assumed by small fragments of mercury.

These phenomena and others are found to be explained by the fact that the surface of these liquids is always under a slight but constant tension, as if enclosed in a thin elastic membrane.

No satisfactory explantion as to the cause of this surface-tension has, I believe, been as yet found; but the fact itself is proved beyond all question. It is a molecular phenomenon, and in order to offer any explanation as to its cause it would be necessary to adopt some hypothesis respecting the molecular constitution of the liquid. Such an explanation making the surface-tension to arise from the cohesion of the molecules of the liquid is, I believe, possible; but this is beside my present purpose, which will be completely served by showing that

The Surface-Tension proves the existence of Cohesion.

To prove this requires no molecular hypothesis, but before proceeding it may be well to define clearly the term cohesion.

Cohesion in a liquid is here to be understood as a property which enables the fluid to resist any tendency to cause internal separation of its parts—any tendency to draw it asunder, or more definitely, it is the property which enables a liquid to resist a tension or negative pressure.

Let us suppose a mass of liquid without internal cohesion. Then any external action tending to enlarge the capacity within the bounding surface of the liquid would at once cause the interior of the liquid to open, and a hollow would be formed within the liquid without any resistance on the part of the liquid. Such a condition is inconsistent with surface-tension, for the tension of the surface of the internal hollow would tend to contract the hollow; and since the interior of the hollow is supposed to be empty, there could be no resistance to the tendency of the surface to contract such as that offered by the pressure of the gas within an ordinary bubble. Hence any force that might, under the circumstances, balance the surface tension and keep open the hollow, must be supplied by the suction or cohesion of the liquid outside.—Q.E.D.

Again; *the intensity of cohesion is determined by the intensity of the surface-tension, and the smallness of the least possible opening over the surface of which tension exists.*

In so far as has yet been determined by experiment, it has been found that the surface-tension is independent of the curvature of the surface—is constant for the same liquid. Assuming that this is the case it follows, that the intensity of the force necessary to keep a spherical bubble or opening from contracting (whether this force arises from the pressure of the gas within the bubble or the cohesive traction of the liquid without the opening) is equal to twice the intensity of the surface-tension divided by the radius of the sphere. *Hence the cohesive tension must be equal to twice the surface-tension of the liquid divided by the diameter of the smallest opening for which the surface-tension exists.*—Q.E.D.

It immediately follows from the foregoing proposition, that no matter how small the surface-tension may be, provided it is finite, even when the opening is infinitely small, then the cohesion of the liquid must be infinitely great. For if the liquid were continuous in its origin the opening must always be infinitely small; and hence to cause such an opening would require infinite tension.

That the cohesion is infinitely great is not probable, to say the least. Hence it is improbable that the surface-tension remains finite when the opening becomes infinitely small. As has already been stated, it has been found that the surface-tension is constant, or nearly so, under ordinary circumstances; but it has never been measured for bubbles of very small diameter, and there appears to be every probability that when the size of the bubble comes to be of the same order of small quantity as the dimensions of a molecule the surface-tension must diminish rapidly with the size of the bubble.

If this is the case, then we have a limit to the cohesion, although it is probably very great for most liquids. Something like the cohesion of solid matter of the same kind. That is to say, it is probable that it would require nearly as great intensity of stress to rupture fluid as it would to rupture solid mercury; or as great tension to rupture water as to rupture ice.

The Effect of Vapour.

Nothing has yet been said about the effect of the pressure of vapour within the bubbles in balancing the surface tension. It may, however, be shown that this can be of no moment. Even supposing that the tension of the vapour within the opening of the liquid were equal to the tension due to the temperature under ordinary circumstances, this would be inappreciable. So that unless the tension of vapour within small openings were much greater than that in larger openings for the same temperature, its effect might be neglected; and so far from this being the case, Sir William Thomson has shown that the pressure of the vapour within a bubble at any particular temperature diminishes with the size of the opening. Hence it is clear that this vapour can have no effect on the result—a conclusion verified by the now well known fact that water may be raised to a temperature high above 212° without passing into steam.

Experimental Verification necessary.

This line of reasoning has been apparent to me now for several years. I find notes on some of the principal points which I made in 1873, and for several years I have pointed out the conclusions arrived at as regards the probable cohesion of water to the students in the engineering class at Owens College. I have, however, hitherto refrained from publishing my views, because I had no definite experimental results to appeal to in confirmation of them. Experimental indications of such a cohesive force were not wanting, but they were not definite. And although methods of making definite experiments have often occupied my thoughts, certain difficulties, which turn out to have been somewhat imaginary, kept me from trying the experiments.

It had always appeared to me that in order to subject the interior of a liquid mass to tension, it would be necessary to, as it were, hold the surface of the liquid at all points to prevent its contracting. To accomplish this it was necessary to have the liquid in a vessel, to the surface of which the liquid would adhere as water adheres to glass. The experiment which I had conceived would have been equivalent to a vertical glass tube more than 32 feet long, closed at the upper end and open at the lower, so that when the tube was full of water the column would be higher than the pressure of the atmosphere would maintain, and hence could only be maintained by the cohesion of the water. The difficulty of such an experiment, however, appeared to be great. It was clear that if mercury could be substituted for water this difficulty would be much reduced, but then mercury does not readily adhere to glass, and the ordinary method of making barometers seemed to disprove the possibility of making it adhere.

It was only on the 2nd of this month that an accidental phenomenon at once afforded me the experimental proof for which I had been looking.

First Experiments.

The phenomenon was observed in a mercurial vacuum gauge (a syphon gauge, which allowed of a column of mercury 31 inches long). Before the mercury was introduced the tube had been wetted with sulphuric acid, a few drops of which covered the mercury on both ends of the column.

The gauge had been in constant use as a vacuum gauge for three weeks, and, probably owing to the action of the acid on the mercury, a little gas had generated between the mercury and the closed end of the tube, sufficient to cause the column to sink to 27½ when the barometer stood at 29. To get rid of this air the tube was removed from its situation and placed in such a position that the bubble of air passed along the tube and escaped, the open end of the tube being entirely free. Before the tube was tilled in this way the unbalanced column was 27½ inches long. When tilled the mercury ran back right up to the end of the tube as the bubble of air passed out. On erecting the tube again the mercury remained up to the end of the tube, except about one-eighth of an inch which filled with sulphuric acid. The unbalanced column of mercury was therefore 31 inches long. At first the full significance of this phenomenon was not recognised,.

but in order to ascertain that the tube was cleared of air it was moved gently up and down to see if the mercury clicked, as it usually does when the tube is free from air, but the mercury did not move in the tube. The rapidity of the oscillation was thereupon increased until it became a violent shake, and as the mercury still remained firm it was clear that some very powerful force was holding it in its place. The tube, being in a vertical position, was then left in order that the barometer might be consulted. This was standing at 29 inches. After a few seconds, when the gauge was again examined, the column no longer reached the end of the tube, but stood at 29 inches. As it was singular that the mercury should have quietly settled down after having resisted such violent shaking, the tube was again inclined until the mercury and acid came apparently up to the end of the tube; but this time on the erection of the tube the mercury at once settled down. That is to say, it settled down gradually as the tube was erected. At first what appeared to be a very small bubble opened in the sulphuric acid, and this enlarged as the top of the tube was raised. On again inclining the tube until it was horizontal, and examining it closely, a minute bubble could be seen in the acid, and it was this bubble which expanded as the tube was erected, and so allowed the mercury to descend. To get rid of this bubble the tube was turned down so as to allow the bubble to pass along the tube, but owing to its small size it would not pass many inches along the tube before it became fixed between the mercury and the glass. When the bubble came to a standstill at about 6 inches from the end of the tube the gauge was again erected, the bubble immediately began to move back, but so slowly that it was some seconds before it entered the region of no pressure. During this interval the mercury remained up to the end of the tube, but the bubble, as soon as it neared the top of the tube, expanded and rapidly rose to the top of the tube, leaving the column at 29 inches. This operation having been repeated several times, it became quite evident that it was this small bubble which either by rising up the tube or being generated at the top had caused the mercury in the first instance to sink. As the bubble would not pass out by itself the tube was tilted so as to allow a larger bubble of air to enter, and having been left standing for about 12 hours to allow the small bubble to unite with the larger one, it was again tipped so as to allow the air to pass out. When this was done the mercury again remained firmly against the end of the tube and did not descend when violently shaken. The open end of the tube was then connected with an air pump and exhausted until the pressure within it fell to about 4 inches of mercury. This operation occupied some seconds, but all this time the mercury did not move from the end of the tube, but eventually the column opened near the bottom of the tube and a large bubble appeared which rose up the tube, the mercury falling past through the opening. That the breaking of the column so near the bottom of the tube was owing to the presence at that point of a small bubble of air was almost proved by the fact that on readmitting the air to the open end of the tube and inclining the tube to see if it was free from air, there was found a minute bubble which played exactly the same part as the small bubble which had been previously examined.

At the instant previous to the rupture of the column at the bottom of the tube there must at the top of the tube have been an unbalanced tension or negative pressure equal to 27 inches of mercury, and this tension did not break the continuity of the column. Hence I had a proof that the cohesion within the mercury and the sulphuric acid as well the adhesion of the sulphuric acid to the mercury and the glass is sufficient to resist this very considerable tension.

Further Experiments

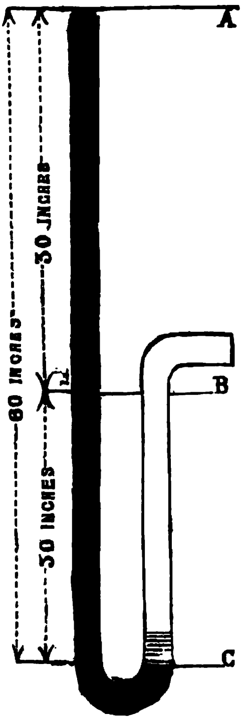
In the hope of improving the experiments another gauge was constructed, the tube being $\frac{6}{16}$ths of an inch in internal diameter, and 35 inches high. Into this tube mercury and sulphuric acid were introduced, as in the first tube. But on trying to get rid of the small bubbles of air, it was found impossible to do so as small bubbles were continually generated. Hence it appeared that the three weeks during which the mercury and sulphuric acid in the first tube had remained in contact had had an important influence on the result. Failing in this attempt, it occurred to me to try if water would answer the purpose as well as sulphuric acid. Having in my possession an old vacuum gauge, with a column 3 inches long, which had originally been wetted with sulphuric acid, but into which a considerable quantity of water had accidently been introduced, I carefully allowed all the air to escape, and then applied a mercurial air pump to the open end of the gauge, and exhausted as far as the pump would draw. The mercury did not descend. As I could apply no further tension I shook the gauge up and down, but still the mercury remained unmoved. I then tapped the gauge smartly on the side, the mercury then fell 3 inches, until it was level. Having succeeded so far, I extracted the mercury and sulphuric acid from the 35 inch gauge and introduced some water without washing the tube, and having boiled the water in the tube, again introduced the mercury.

Having extracted all the air, I found no difficulty in making the gauge to stand up to the 35 inches without any immediate tendency to fall. On applying the air pump to the open end the mercury several times remained up until the exhaustion had proceeded so far that when it fell it fell from 22 to 28 inches, and when the rupture took place it was accompanied with a loud click. I could not on that occasion get the mercury to withstand complete exhaustion, but after leaving the gauge with the mercury suspended for 24 hours at 35 inches, I was able to exhaust the open end of the tube as far as the pump would draw, without bringing the mercury down, so that I had a column of 35 inches of mercury suspended by the cohesion of the liquids.

There was no reason to suppose that this was the limit or anywhere near the limit. It was clearly possible to suspend a longer column, but as the length of the column increased so would the difficulty of getting rid of the disturbing causes, and I determined to rest satisfied with the 35 inches; but in order to see if this could be maintained, I obtained a gauge 60 inches long, so that this would leave 30 inches above the pressure of the atmosphere.

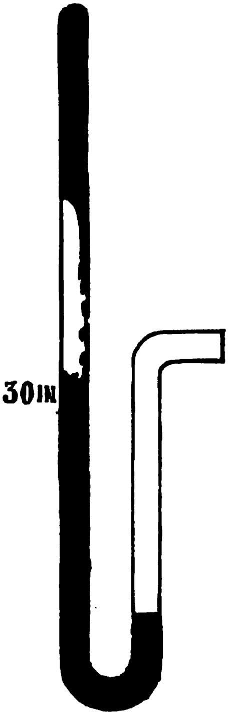
The difficulty of getting rid of the air in this tube sufficiently to allow of the mercury standing 60 inches was very considerable. Before filling the tube it was rinsed out with concentrated sulphuric acid, then twice washed with distilled water, and then water put in and boiled in the tube. Then sufficient mercury was introduced to fill the long leg and the bend, so that the column when complete was 59 inches long, the barometer being at 29·5.

FIG. 3.



After the tube had been tilted several times so as to allow the air to pass out, the mercury would be suspended as the tube was slowly re-erected, until it had attained an elevation of 40, 50, or sometimes the full height, as shown in Fig. 3; but only for a few seconds. When the mercury fell, if the column broke anywhere near the top of the tube, it gave way with a loud click. But this was by no means always the case. The mercury would sometimes sepa-

FIG. 4.



rate nearly 30 inches down the tube, and then the appearance of the upper portion falling was very singular. The upper portion of the column remained intact, and a stream of mercury fell from its under surface, as shown in Fig. 4, breaking up into globules as it came into contact with the lower portion with a loud rattling noise. I was unable to get the column in the tube thus filled to maintain itself for more than 20 or 30 seconds, which failure was clearly due to the presence of air, for after the mercury had fallen a small quantity of air was always found to collect above it. Sometimes when, on inclining the tube, the liquid again reached the top, the bubble which remained was so small as to be scarcely visible, although subject to no pressure other than the surface tension, but its presence always became apparent instantly on erecting the tube. In no case was it possible, after the mercury had once fallen, to get it to remain up to, any considerable height above that due to the pressure of the atmosphere until the bubble of air collected had been allowed to pass out.

The tube was then again emptied, washed, and filled with glycerine. This behaved much in the same manner as the water, but the difficulty of getting rid of the air was greater.

Similar results were obtained when very dilute ammonia liquid was tried.

The tube was then again carefully washed, first with water and then several times with concentrated sulphuric acid. The mercury was subjected to nitric acid, washed and dried, and then filtered into a bottle of sulphuric acid, from which it was poured into the tube, some acid passing in with the mercury. When first introduced into the tube a few small bubbles could be seen rising between the mercury and the tube and passing up through the sulphuric acid into the vacuum above; but after it had stood for five or six hours no bubbles were perceived, the surface of the mercury against the tube being perfectly clear; nevertheless, on erecting the tube the mercury would not rise above the height of the barometer, and air was always found to have collected above the mercury. Water was then introduced so as to dilute the acid. Then the mercury was suspended as before only for a few seconds. The tube was then placed in a position with the closed end lowest, so that the air and water might ascend towards the end and pass out, and after being in this position for some hours, when it was again erected, the column remained intact.

It was thereupon again lowered, and left to drain for forty-eight hours. On being again erected the mercury was still suspended. The tube has since been carried in a more or less horizontal position some three miles to the Society's rooms, in order that I might exhibit the phenomenon. If it has not been affected by the shaking you will see a column of mercury suspended some fifty-nine inches, or twenty-nine inches above the height due to the atmosphere.*

Conclusion.

The difficulty of obtaining a column of mercury 30 inches above the pressure of the atmosphere does not, I think, prove that the limit of the cohesive power of the liquid has been arrived at, or even the limit of the adhesive power of the water for glass and mercury; but simply shows that, although imperceptible, there are still bubbles of air in the liquid between the mercury and the glass which will not readily pass out.

It seems to me to be probable that with sufficient care, or by using apparatus more suitable to the purpose, much greater heights might be attained. But however this may be, we have proof that mercury and water will by their cohesion resist a tension of at least one atmosphere, or that the common pump would, if the water were free from air, raise water by suction to a height of more than 60 feet. At first sight it cannot but appear remarkable that such a fact should for so long have escaped notice, but a little consideration removes the difficulty.

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*At the meeting not only did the mercury remain suspended when the tube was erect, but on the pressure of the atmosphere being removed with an air pump it still remained suspended, although the tension at the top of the tube was nearly equal to two atmospheres.

Water is almost always more or less saturated with air, which separates into bubbles as soon as the pressure is relieved, and in the common pump a single minute bubble would be sufficient to cause the column to break, and prevent it being raised to a greater height than that due to the pressure of the atmosphere.

In the case of barometers it is the custom to fill the tubes full and boil the mercury, so as to get rid of the air; but the column falls to the usual height not by the rupture of the mercury, but by the separation of the mercury from the glass, for which it has but little adhesion. Whether the ordinary method of boiling the mercury really disengages all the air is, I think, an open question. In vacuum gauges of small diameter it is not uncommonly found that the mercury sticks to the glass until the pressure has fallen considerably below what is represented by the height of the mercury, so that on the gauge being shaken the mercury falls with a sudden drop. Although it does not seem to have attracted any especial notice, this phenomenon is clearly due to the same cause as that which I have found capable of maintaining 30 inches of mercury suspended in a comparatively large tube.

It would seem then that although the facts which I now bring before the Society have little bearing on the practical limits to the height of the column of mercury in the barometer or the column water in the common pump, they show that these limits are owing to the presence of air or some other minor disturbing cause, and are not, as seems to have been hitherto supposed, owing to the want of cohesion in the liquid. And it seems to me that the cohesion now found to exist occupies an important as well as interesting place in the properties of liquid.

Appendix, 26th April.

Previous Notices of the Cohesion of Liquids.

Besides the hanging of mercury in small gauges, another phenomenon which has long been known, shows a small degree of cohesion in water; this is, that water will rise up small tubes by capillary attraction as well in the receiver of an air-pump as in air at the ordinary pressure. This fact was shown before the Royal Society by Robert Hooke.

Prof. Maxwell, in his Treatise on the Theory of Heat, p. 259, after commenting on the fact that water has been raised to a temperature of 356° F. without boiling, remarks: "Hence the cohesion of water must be able to support 132lbs. weight on the square inch." From which it would appear that he recognises cohesion as a property of water, and considers that the possibility of raising the temperature above the boiling point is evidence of such cohesion, but I am not aware that he has anywhere given his reasons for such a conclusion.

I am indebted to Dr. Bottomley for reference to a paper in the "Ann. de Chim. et de Phys. III., XVI., 167," by M. F. Donny, in which M. Donny gives an account of experiments in which he found that columns of sulphuric acid could be suspended in vacuo to a height of 1·3m., about 50 inches, showing a tension of about 7 inches of mercury, care having been taken first to remove all the air from the acid. M. Donny further describes experiments made with water in exhausted tubes, in which he showed the effect of cohesion by shaking the tube. M. Donny does not, however, appear to have thought of the plan which I adopted of making mercury adhere to the tubes by wetting them with sulphuric acid or water. Not being able to use mercury the tensions which he obtained were comparatively small, and although he seems to have considered that greater tensions might be obtained he mentions one or two atmospheres as probably possible. It would therefore appear that he had not conceived the possibility of the cohesion of liquids being comparable with that of solids.

M. Donny appears to have been influenced in adopting this limit to his idea of cohesion by a passage from Laplace, *Mécanique Celeste,* Supplement au X^e livre, p. 3, which he quotes.

Laplace, who was the first to investigate systematically the phenomena of capillary attraction, proceeded on the hypothesis that the molecules of a liquid exercise attraction for each other at insensible distances only, and from this assumed attraction he deduces the surface phenomena. The entire passage quoted by M. Donny is too long to introduce here, but the gist of it is comprised in the following extract: —

"*Son expression analitique est composée de deux termes: le premier, beaucoup plus grand que le second, exprime l'action de la masse terminée par une surface plane; et je pense que de ce terme dependent la suspension du mercure dans un tube de baromètre à une hauteur deux ou trois fois plus grande que celle qui est due à la pression de l'atmosphère, le pouvoir refringent de corps diaphanes, la cohesion, et généralement les affinités chimiques.*"

Laplace here speaks of the suspension of mercury to 60 or 90 inches as if it were a well known phenomenon; but I cannot find any reference to experiments, or indeed any further mention of the phenomenon, in his memoir.

I did not refer to Laplace in the first instance, although I knew well that it is to him we are indebted for the theory of surface tension almost in the form now accepted, because I wished to avoid all reference to molecular hypothesis, and particularly the molecular attractions assumed by Laplace, lest it might in any way appear as if the conclusion that continuous liquids are as capable of resisting tension as solids, at which I arrived simply from considering the phenomena of surface tension was based on such assumptions. I was not aware, however, that Laplace had at all inferred or attempted to apply his theory to prove the ability of liquids to resist great tensions, nor do I find on again reading his memoir that he anywhere, with the exception of the almost casual reference quoted above, treats of such a property of liquids. His purpose appears to have been solely to explain the phenomena of capillarity. It appears obvious, moreover, that his line of reasoning must have forced upon his notice the conclusion that according to his hypothesis liquids ought to possess the property of very great cohesion, so that from the extremely slight notice which he has accorded to this property, one can only infer that he was not completely convinced of its existence.

"On the Estimation of Hyposulphites and Sulphites," by J. GROSSMANN, Ph.D. Communicated by Professor ROSCOE F.R.S.

Bunsen, in his treatise "On a Volumetric Method of very general applicability," Ann. Chem. & Pharm., has shown what degree of exactness may under certain circumstances be attained by indirect analysis. Upon the latter I have founded a very simple method for the estimation of hyposulphites and sulphites, which yields exact results. I shall speak in the following only of the sodium salts of the above compounds, as it will make the explanation clearer.

Let us assume first that we have only a mixture of sodium hyposulphite and sulphite, free from sulphate, in solution. If on the one hand we estimate how many grammes of iodine are required by an aliquot part of the mixture—acidulated with acetic acid—to yield the well known final blue reaction with starch, and if, on the other hand, we find how many grammes of sodium sulphate an equal measure of the mixture yields on perfect oxidation with chlorine or bromine, all the data necessary for the calculation of the respective quantities of hyposulphite and sulphite are in our possession, and we can form two equations from which both the unknown quantities x_1 and y_1 can easily be calculated.

Let x_1 = grammes iodine corresponding to $Na_2S_2O_3$ present,
y_1 = grammes iodine corresponding to Na_2SO_3 „
A = the quantity of iodine which the unit requires for testing with starch.
and B = grammes Na_2SO_4 which the unit yields on perfect oxidation,

then

$$x_1 + y_1 = A$$

$$\frac{2Na_2SO_4}{Na_2S_2O_3} \cdot \frac{Na_2S_2O_3}{I} \cdot x_1 + \frac{Na_2SO_4}{Na_2SO_3} \cdot \frac{Na_2SO_3}{2I} \cdot y_1 = B$$

or

$$\frac{2Na_2SO_4}{I} x_1 + \frac{Na_2SO_4}{2I} \cdot y_1 = B$$

which gives us

$$x_1 = \frac{2I}{3Na_2SO_4} \cdot B - \tfrac{1}{3}A$$

$$y_1 = \tfrac{4}{3}A - \frac{2I}{3Na_2SO_4} \cdot B.$$

To get the result as grammes $Na_2S_2O_3$ and Na_2SO_3 it is only necessary to multiply x_1 by $\frac{Na_2S_2O_3}{I}$, and y_1 by $\frac{Na_2SO_3}{2I}$. From this we find

$$x = \text{grammes } Na_2S_2O_3 = \frac{2Na_2S_2O_3}{3Na_2S\,O_4} \cdot B - \frac{Na_2S_2O_3}{3I} \cdot A$$
$$= \cdot 741784\ B - \cdot 414698\ A.$$

$$y = \text{grammes } Na_2SO_3 = \frac{2Na_2SO_3}{3I} \cdot A - \frac{Na_2SO_3}{3Na_2SO_4} \cdot B$$
$$= \cdot 661417\ A - \cdot 295775\ B.$$

Care must of course be taken that the dilution while titrating is sufficient, and at least two titrations ought to be made.

If besides hyposulphite and sulphite, sulphate is present it is necessary to estimate this by itself, and the Na_2SO_4 thus found subtracted from the total Na_2SO_4 is B. As long as there is no sulphide present this can be done in the following manner. The liquor to be examined is heated in a flask and carbonic acid passed through till all the air is expelled. Then hydrochloric acid is added in excess, and the liquid boiled down in a CO_2 atmosphere to one fourth of its volume. After filtering it is precipitated with barium chloride as usual.

·21385 grammes Na_2SO_4

were mixed with a very great excess of $Na_2S_2O_3$ and Na_2SO_3 and yielded after being treated as described above

·2151 grammes Na_2SO_4

If sulphide is present this must be got rid of by means of cadmium carbonate before starting the analysis.

A mixture was prepared containing per unit

Na_2SO_4	·4165	grammes	= 39·06
$Na_2S_2O_3$	·3946	„	= 37·01
Na_2SO_3	·2551	„	= 23·93
	1·0662		100·00

and the following data got

Total Na_2SO_4......1·4174 grammes
Actual Na_2SO_4...... ·4165 „

therefore $B = 1{\cdot}0009$
$A = {\cdot}833189$

from which we calculate

Na_2SO_4	·4165	grammes	= 38·98
$Na_2S_2O_3$	·3969	„	= 37·15
Na_2SO_3	·2550	„	= 23·87
	1·0684		100·00

I have used this method with great advantage in the analysis of intermediate as well as final products in the manufacture of soda ash and caustic soda.

Laboratory of Gerard's Bridge Chemical Works,
St. Helens.

"Note on the Action of Iodine Trichloride upon Carbon Bisulphide," by J. B. HANNAY, F.R.S.E., F.C.S.

It is stated by Weber that when iodine trichloride is added to carbon bisulphide a new product is formed. Now I have examined the action of these two substances upon each other, and find it is according to the following equation:—

$$2CS_2 + 3ICl_3 = CCl_4 + CSCl_2 + 3SCl + 3I.$$

On adding carbon bisulphide to pure iodine trichloride till it was all decomposed, considerable heat was evolved, and on cooling, iodine crystallized out. A qualitative examination of the liquid showed the presence of sulphur chloride in large quantities, and on decomposing this with water, the characteristic smell of the sulphochloride of carbon was observed. This was further recognised by its deportment with alkalies, and after its removal from the liquid, the tetrachloride of carbon was easily recognised by its peculiar sweet smell strongly reminding one of the smell of primrose leaves.

A weighed portion of the trichloride was heated with the requisite amount of carbon bisulphide to convert it into the above products, when it was found there was neither an excess of carbon bisulphide nor of the trichloride present. The liquid was allowed to cool, and when all the iodine had crystallized out, it was passed through a small filter of asbestos and washed with a few drops of carbon tetrachloride. The iodine on the filter was estimated and gave only a little under the required amount. The filtrate was then treated with caustic potash, and the separated carbon tetrachloride weighed after transference to a tared bulb. The sulphur in the liquid, left after removal of the tetrachloride of carbon, was oxidised by means of potassium chlorate and strong nitric acid, and estimated as barium sulphate.

The following table gives the figures which were obtained on working with 10 grammes of iodine trichloride, and also those deduced from the above equation:—

	Found.	Calculated.
CCl_4	2·05	2·198
$CSCl_2$	1·71	1·641
CS_2Cl_2	2·90	2·890
I	5·37	5·428

"Note by A. W. WATERS to his paper on the Bryozoa, page 125."

At the time I wrote my paper I was unable to consult Thompson's "Zoological Researches," but upon referring in London I find that as the quotation given relates to the Flustraceæ it would have been better to have given the following from page 97: "The other species of Sertularia in which the animals have been determined to be Polyzoæ."

Also on page 132 of my paper read for "cœnosare"—"endosare," which term is used by Joliet for the first time.

April 11th.

Annual General Meeting, April 30th, 1878.

E. W. **Binney**, F.R.S., F.G.S., President, in the Chair.

Mr. William Dugdale Harland, of Manchester, was elected an Ordinary Member of the Society.

Report of the Council, April, 1878.

It will be seen from the Treasurer's Annual Statement appended to this report that there is apparently a balance against the General Account of £10 14s. 5d.; but this is much more than covered by rents which are due from the Geological Society and the Scientific Students' Association for the use of the Society's rooms.

The number of ordinary members on the roll of the Society on the 1st of April, 1877, was 162, and three new members have since been elected; the losses are—deaths 4; resignations 4; defaulter 1. The number on the roll on the 1st instant was 156. The deceased members are Mr. Colin Mather, Mr. G. V. Vernon, F.R.A.S., Mr. W. L. Dickinson, and Mr. John Thomas Hobson.

Mr. George Venables Vernon was the only son of Mr. John Venables Vernon, of the firm of Vernon and Edge, engravers to calico printers, David-street, Manchester, and was born on the 7th of October, 1831. He received a good education, and appears to have displayed a strong taste for scientific pursuits at an early age. On the 14th of January, 1853, he was elected a Fellow of the Royal Astronomical Society, and on the 30th of April, 1861, he was elected a Member of this Society, and afterwards contributed many valuable papers and communications, chiefly on meteorological subjects, to its Proceedings and Memoirs. He served as a member of the council, and was for several years Honorary Secretary of the Physical and Mathematical Section. He was a Fellow of the Meteorological Society, and of the Anthropological Society, and a Member of the Meteorological Society of Scotland, and of the Meteorological Society of France.

Mr. Vernon carried on the business of a cotton spinner until his death, which occurred on the 11th of January, 1878, at the age of 46 years.

In private life Mr. Vernon was genial and kind-hearted and he was very fond of music and other accomplishments.

William Leeson Dickinson was the eldest son of Robert Dickinson, mercer, of Nottingham, and Mary his wife, eldest daughter of William Leeson, of Halam, near Southwell, gentleman, and was born on the 15th of August, 1814. He was educated at Arnold Vicarage, near Nottingham, by the Rev. William Howard, and afterwards apprenticed to Mr. George Gamble, of Gainsbro', chemist. Upon attaining his majority he went to Manchester, and was for some time assistant to the late Mr. Hargreaves, chemist, Market-street, upon leaving whom he commenced business as a drysalter. From his boyhood he was passionately fond of music, and took great delight in the study of mathematics, astronomy, and statistics. He was elected a member of the Society on the 23rd January, 1855, and afterwards contributed the following papers and communications to the Society's Proceedings:—

Eclipse of the Sun, March 15, 1858	Vol. I., p.	7
Eclipse of the Sun, July 18, 1860	„	206
Occultation of Jupiter, May 24, 1860	„	244
Note on the Transit of Mercury, Nov. 12, 1861	„ II.,	151
On the Eclipse of the Sun, Dec. 31, 1861	„	152
Eclipse of the Sun, May 17, 1863	„ III.,	66
Eclipse of the Sun, Oct. 10, 1865	„ V.,	2
Eclipse of the Sun, Oct. 8, 1866	„	170
Occultation of Aldebaran, Sep. 28 and Nov. 22, 1866	„	170

Occultation of Aldebaran, Jan. 18, 1867 …………	Vol. VI.,	47
On the Eclipse of the Sun, March 5, 1867 ………	„	107
On three Occultations of Saturn by the Moon ……	„ IX.,	149
On the Eclipse of the Sun, Dec. 21-22, 1870 ……	„	158

On the 11th of October, 1871, Mr. Dickinson had a paralytic attack, from which time he never was able to return to his warehouse; he was removed to Halam on the 15th of March, 1872, and died February 23rd, 1878.

John Thomas Hobson, Ph.D., F.C.S., was born in Manchester and educated mainly at Owens College, where he obtained the Dalton Scholarship. He afterwards studied in Marburg under Professor Kolbe, and took there his degree. He spent several years as chemist with the firm of Thomas Hoyle and Sons, and in 1863 he was appointed sub-inspector under the Alkali Act. To the duties of this office he gave all his attention, until an accident broke down his vigour, which after about ten years' illness ended in his death in December, 1877.

The Natural History Fund of £1,500, presented by the late Natural History Society, has been received from the Trustees of the Owens College by the Treasurer, and the Council have given instructions for its investment in any of the stocks or securities authorised by the trust deed.

The following papers and communications have been read at the Ordinary and Sectional Meetings of the Society during the session:—

October 2nd, 1877.—"A Case of Flowering of *Chaemerops Fortunei* (Hook), at Alderley," by Arthur W. Waters, F.G.S.

"Table of Effect of Movement of the Surface of the Globe on the Shifting of the Axis of the Earth," by Arthur W. Waters, F.G.S.

October 8th, 1877.—"Address on the Recent Progress of Natural History," by Professor W. Boyd Dawkins, F.R.S., &c.

October 16th 1877.—"On some Coal Measure Plants and other Organic Remains from Spain," by E. W. Binney, F.R.S., F.G.S., President.

"Note on an Edible Clay from New Zealand," by M. M. Pattison Muir, F.R.S.E.

"On the Decomposition of Calcium Sulphate by Alkaline Chlorides. A contribution to Agricultural Chemistry," by M. M. Pattison Muir, F.R.S.E.

"On Some Thionates," by H. Baker, student in the Owens College. Communicated by Professor C. Schorlemmer, F.R.S.

November 5th, 1877.—"On the Position and Affinities of the Genus Rostellaria (R. Martinii Marrat)," by J. Cosmo Melvill, M.A.

November 13th, 1877.—"Note on Envelopes and Singular Solutions," by Sir James Cockle, F.R.S., Corresponding Member of the Society.

"On the Formation of Hailstones, Raindrops, and Snow-flakes," by Professor O. Reynolds, F.R.S.

November 27th, 1877.—"On the Origin of some Ores of Copper (Part I.)," by Charles A. Burghardt, Ph.D., the Owens College.

"On the Construction of a Room, or Series of Rooms, free from Germ Life, proposed for use in the performance of Surgical Operations," illustrated by a model room and apparatus, by William Thomson, F.R.S.E.

December 4th, 1877.—"Transit of the Shadow of Titan across the Disc of Saturn, November 23rd, 1877," by Joseph Baxendell, F.R.A.S.

December 11th, 1877.—"Note on the Daguerreotype Portrait taken of the late Dr. Dalton," by J. B. Dancer, F.R.A.S.

"Note on Metallic Niobium and a New Niobium Chloride," by Professor H. E. Roscoe, F.R.S., &c.

"On the Retention of Saline Impurities by Hydrated Ferric Oxide," by Mr. Charles Frederick Cross, Dalton Scholar in the Owens College. Communicated by Professor H. E. Roscoe, F.R.S.

December 26th, 1877.—"Notice of a Large Boulder Stone at Old Trafford, Manchester," by E. W. Binney, F.R.S., F.G.S., President.

"On the Geometrical Representation of the Equation of the Second Degree," by Charles Chambers, F.R.S., Superintendent of the Colaba Observatory, Bombay. Communicated by J. A. Bennion, F.R.A.S., A.C.P.S.

January 8th, 1878.—"On a Large Meteor Observed on the 31st of December, 1877," by E. W. Binney, F.R.S., F.G.S., President.

"Note on the Decomposition of Water by Iron-pyrites," by C. A. Burghardt, Ph.D.

"On a Remarkable Japanese Picture," by R. D. Darbishire, F.G.S.

"On the Microscopic Conditions of a Slab from the Mountain Limestone of Bolland," by Professor W. C. Williamson, F.R.S.

January 14th, 1878.—"The Mollusca of Cymmeran Bay, Part IV.," by John Plant, F.G.S.

January 22nd, 1878.—"On the Cubic Integral

$$u = \int_{\beta}^{\alpha} \frac{dx}{\sqrt{(a-x)(b-x)(c-x)}},"$$

by Robert Rawson, Esq., Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society; Mem. of the London Math. Society.

"Notice of a Fossil Plant found at Laxey, in the Isle of Man," by E. W. Binney, F.R.S., F.G.S., President.

"On Ptolemy's Geography, with Reference to the Coast from Caernarvon to Cumberland," by Thomas Glazebrook Rylands, Esq. Communicated by Dr. R. Angus Smith, F.R.S., &c.

February 5th, 1878.—"On an Improved Method of Projecting Lissajous' Figures on the Screen," by J. Dixon Mann, L.K.Q.C.P.

February 11th, 1878.—"On the Best Method of Collecting and Preserving Plants for Herbarium Purposes, when gathered in Tropical or Subtropical Climates," by H. A. Hurst, Esq.

February 19th, 1878.—"Note on a Method for Determining the Coefficients in Chemical Equations," by James Bottomley, D.Sc.

February 25th, 1878.—"Results and Deductions of Rain-gauge Observations made at Eccles, near Manchester, during the year 1877," by Thomas Mackereth, F.R.A.S., F.M.S.

March 5th, 1878.—"On the Decomposition of Ultramarine by Carbonic Acid," by M. S. Sugiura (student in the Chemical Laboratory of the Owens College). Communicated by Professor Roscoe, F.R.S., &c.

"On Silicious Fossilization," by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Professor Roscoe, F.R.S., &c.

March 11*th*, 1878.—"On Bryozoa," by Arthur Wm. Waters, F.G.S.

"On Metallic Growths on the Surfaces of Specimens of Auriferous Quartz from Australia and North Wales," by John Plant, F.G.S.

March 19*th*, 1878.—"On a Remarkable Flash of Lightning," by B. St. J. B. Joule, Esq.

"On a Barometer," by Dr. J. P. Joule, F.R.S., &c.

"On a New Form of Portable Sprengel Pump," by C. H. Stearn, Esq.

"A Comparison of the Standard Barometer of the Owens College Physical Laboratory with the Working Barometer," by M. Morisabro Hiraoka, student of Owens College. Communicated by Professor B. Stewart, LL.D., F.R.S.

"On a New Calorimeter," by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry, Owens College.

April 2*nd*, 1878.—Three Letters from the late Sir William Fairbairn, Bart., F.R.S., a former president of the Society, to the late Professor Hodgkinson, F.R.S., also a former president of the Society. Communicated by E. W. Binney, F.R.S., F.G.S., President.

"On Aurin," by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

"The Origin of some Ores of Copper (Part II.)," by Charles A. Burghardt, Ph.D., the Owens College.

April 16*th*, 1878.—"On Indigo-blue from Polygonum tinctorium and other Plants," by Edward Schunck, Ph.D., F.R.S.

"On the Internal Cohesion of Liquids," by Professor Osborne Reynolds, M.A., F.R.S.

"On the Estimation of Hypo-sulphites and Sulphites," by F. Grossmann, Ph.D. Communicated by Professor Roscoe, F.R.S.

"On the Action of Iodine Trichloride on Carbon Disulphide," by J. B. Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College. Communicated by Professor Roscoe, F.R.S., &c.

Many of the above papers have already been printed in the new volume of Memoirs, and others have been passed by the Council for printing.

The Council consider it desirable to continue the system of electing Sectional Associates during the ensuing session, and a resolution on the subject will be submitted at the annual meeting for the approval of the members.

The Librarian reports that since the last annual meeting the binding of the books has been very nearly finished up to date, and we have completed many of the series of the Societies that were wanting. The Microscopical and Natural History Section have added many standard works to the Library during the year, amongst which are "The Ibis," Hewitson's "Exotic Butterflies," Johnstone's "British Sea Weeds," Semann's "Flora Vitiensis," Hooker's "Niger Flora," Hooker's "Flora of British India," vol. 1, Hooker's "Flora of Tasmania," Booth's "Carex," (last volume), Hawey's "Flora Capensis," Oliver's "Flora of Tropical Africa," &c.

The number of Societies with which we correspond continues about the same as last year.

On the motion of Mr. A. BROTHERS, seconded by Mr. T. MACKERETH, the Report was unanimously adopted, and ordered to be printed in the Society's Proceedings.

On the motion of Mr. A. W. WATERS, seconded by Dr. C. A. BURGHARDT, it was resolved unanimously :—

That the system of electing Sectional Associates be continued during the ensuing Session.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:—

President.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.

Vice-Presidents.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A., F.R.S.

Treasurer.

CHARLES BAILEY, F.L.S.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
CARL SCHORLEMMER, F.R.S.
WILLIAM E. AXON, M.R.S.L., F.S.S.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

harles Bailey, Treasurer, in account with the Society from 1st April, 1877, to 31st March, 1878, with a Comparative Statement of Accounts for the Two Preceding Years.

Dr.

	£ s. d.	1877-8. £ s. d.	1876-7. £ s. d.	1875-6. £ s. d.
7.—April 1.				
To cash in hand		247 19 9	236 19 2	542 1 1
8.—March 30.—				
To Members' Contributions:—				
Arrears, 1875-6, 1 at £2 2s.	2 2 0			
,, 1876-7, 7 at £2 2s.	14 14 0			
New Member's Admission, 1876-7, 1 at £2 2s.	2 2 0			
,, Half Subscription ,,	1 1 0			
,, Admission, 1877-8, 2 at £2 2s.	4 4 0			
,, Subscription ,, ,,	4 4 0			
Old Members, 1877-8, 145 at £2 2s.	304 10 0	332 17 0	358 1 0	344 8 0
To One compounder (Mr. W. H. Johnson)		26 5 0		
Two Associates' Subs. (for Library)	1 0 0			
Sectional Contributions, 1877-8:—				
Physical and Mathematical Section	2 2 0			
Microscopical, Natural Hist. do.	2 2 0	5 4 0	9 8 0	5 4 0
Use of Society's Rooms:—				
Geological Society to 31st Mar., 1877	30 0 0			
Scientific Students' Association, to 31st March, 1877	11 10 0	41 10 0	41 10 0	78 5 0
Sale of Society's Publications		2 10 0	5 6 0	2 6 7
Natural History Fund—Interest from Owens College to 22nd Feb., 1878	68 16 9			
Capital Fund—Natural Hist. Society's donation	1500 0 0	1568 16 9	59 6 3	59 10 0
Bank Interest, less Bank Postage, to 31st December, 1877		4 11 10	3 17 1	8 16 0
		£2229 14 4	£714 7 6	£1040 10 8

Cr.

	£ s. d.	1877-8. £ s. d.	1876-7. £ s. d.	1875-6. £ s. d.
1878.—March 30.				
By Charges on Property:—				
Chief Rent	12 15 2			
Insurance against Fire	13 7 6			
Property Tax	2 2 6			
Repairs, Whitewashing, &c.	2 0 2	30 5 4	23 18 2	146 14 2
By House Expenditure:—				
Coals, Gas, Candles, and Water	18 17 9			
Tea and Coffee at meetings	18 14 6			
House Duty	6 7 6			
Cleaning, Brushes, Sundries	6 2 7	50 2 4	55 0 6	92 3 0
By Administrative Charges:—				
Wages of Keeper of Rooms	57 4 0			
Postages and Carriage of Parcels	26 17 4			
Attendance on Sections and Societies	15 14 0			
Stationery and Printing Circulars	8 9 3	108 4 7	130 10 8	191 14 11
By Publishing:—				
Printing Memoirs	74 4 6			
Printing Proceedings	63 15 0			
Wood Engravings	6 3 3			
Editor of Memoirs and Proceedings	50 0 0	194 2 9	112 1 0	138 1 6
By Library:—				
Binding Books	35 2 6			
Books and Periodicals	11 2 6			
Assistant in Library	11 6 0			
Old Memoirs of the Society	1 12 0			
Geological Record for 1875	0 11 2			
Palæontographical Society for 1878	1 1 0	60 15 2	144 17 5	194 17 11
By Natural History Fund:—				
Grant to Microscopical and Natural History Section for Books		60 0 0		40 0 0
By Balance		1726 4 2	247 19 9	236 19 2
		£2229 14 4	£714 7 6	£1040 10 8

	£ s. d.	£ s. d.	£ s. d.
878.—April 1.—To Cash in hand, viz.:—			
Compounders' Fund		125 0 0	
Natural History Fund:—			
Interest Account—Balance 1st April, 1877	103 1 10		
Interest to 1st April, 1878	68 16 9		
	171 18 7		
Less Grant for Books 20th April, 1877	60 0 0	111 18 7	
ital Account		1500 0	
		1736 18 7	
alance against the General Account		10 14 5	1726 4 2

April 9th, 1878.—Audited and found correct,

(Signed) THOS. MACKERETH,
JAMES BOTTOMLEY.

“The Distribution of Ammonia,” by Dr. R. ANGUS SMITH, F.R.S., &c.

If organic matter is everywhere ammonia is everywhere possible, and if that matter is decomposing, ammonia is everywhere. This is the general statement which this paper illustrates. It is now many years since it was observed by me that organic matter could be found on surfaces exposed to exhalations from human beings; but it is not till now that the full significance of the fact has shone on me, and the practical results that may be drawn from it in hygiene and meteorology. These results are the great extension of the idea that ammonia may be an index of decayed matter; the idea itself has been used partly and to a large extent, as illustrated in my “Air and Rain.” The facts now to be given enable us to claim for it a still more important place. The application seems to fit well the conditions already examined, and by this means currents from foul places have been readily found. This does not apply to the substances which may be called germs, whether it be possible to see them or not, because these are not bodies which have passed into the ammoniacal stage although some of them may be passing; those for example which are purely chemical and exert what we may call *idiolytic* action. This word, from ἴδιος, its own (peculiar), and λύσυς, decomposition, may serve to mark this peculiar action which was left by Liebig unnamed; he used the vague term invented by Berzelius, namely catalytic. I have elsewhere recognised the two classes of germs, instead of any disputed one, without naming them.

It is now many years since Liebig first surprised me by saying that iron ores and aluminous earths were capable of taking up ammonia, and if they were breathed upon we were able even to smell that substance. He, much about the same time, made numerous experiments in order to find the ammonia of the atmosphere, and to measure its amount in rain. The result for science was great, and Professor Way continued the enquiry for the Royal Agricultural Society. Dr. Gilbert, F.R.S., amongst his many labours in the department of agricultural science, has made this enquiry into ammonia of rain in still later times, but I shall not at present quote his results, as this paper does not intend to go fully into the subject, but rather to indicate its magnitude and importance. The first paper I ever read to this Society was on the ammonia found in peat—I was unable then to see the extent of the subject.

I shall give parts of the fuller paper without the long tables of results.

Ammonia must ever be one of the most interesting of chemical compounds. It comes from all living organisms, and is equally necessary to build them up. To do this it must be wherever plants or animals grow or decay. As it is volatile, some of it is launched into the air on its escape from combination, and in the air it is always found. As it is soluble in water it is found wherever we find water on the surface of the earth or in the air, and probably in all natural waters, even the deepest and most purified. As a part of the atmosphere it touches all substances and can be found on many; it is in reality universally on the surface of the earth, in the presence of men and animals, perhaps attached more or less to all objects, but especially to all found within human habitations, and we might also add with equal certainty, the habitations of all animals.

If you pick up a stone in a city and wash off the matter on the surface, you will find the water to contain ammonia. If you wash a chair or a table or anything in a room, you will find ammonia in the washing, and if you wash your hands you will find the same; and your paper, your pen, your table cloth, and clothes, all show ammonia, and even the glass cover to an ornament has retained some on its surface. You will find it not to be a permanent part of the glass, because you require only to wash with pure water once or twice, and you will obtain a washing which contains no ammonia. It is only superficial.

This ammonia on the surface is partly the result of the decomposition of organic matter continually taking place and adhering to everything in dwellings. The presence of organic matter is easily accounted for, but it is less easily detected than ammonia. It is probable that the chief cause of the presence of ammonia on surfaces in houses and near habitations is the direct decomposition of organic matter on the spot. If so, its presence, being more readily observed than organic matter itself, may be taken as a test and the amount will be a measure of impurity. A room that has a smell indicating recent residence will, in a certain time, have its objects covered with organic matter, and this will be indicated by ammonia on the surface of objects. After some preliminary trials, seeing this remarkable constancy of comparative results, and the beautiful gradations of amount, it occurred to me that the same substance must be found on all objects around us whether in a town or not; I therefore went a mile from the outskirts of Manchester and examined the objects on the way. Stones that not twenty hours before had been washed by rain, showed ammonia. It is true that the rain of Manchester contains it also, but considering that only a thin layer would be evaporated from these stones, it was remarkable that they indicated the existence of any. The surface of wood was examined—palings, railings, branches of trees, grass (not very green at the time), all showed ammonia in no very small quantities. It seemed as if the whole visible surface around had ammonia. I went into the house and examined the surfaces in rooms empty and inhabited, tables, chairs, ornaments, plates, glasses, and drawing-room ornaments. A (parian) porcelain statuette, under a glass, showed some ammonia, a candlestick of the same material (but un-covered) showed much more; the back of a chair showed ammonia, when rubbed with a common duster, very little. It seemed clear that ammonia stuck to everything.

If, then, ammonia were everywhere, the conclusion seemed to be that it was not at all necessary to do as I had been doing, namely, wash the air so laboriously; it would be quite sufficient to suspend a piece of glass and allow the ammonia to settle upon it. For this purpose small flasks were hung in various parts of the laboratory, and they were examined daily. The flasks would hold about 6 ounces of liquid, but they were empty, and the outer surface was washed with pure water by means of a spray bottle; it was done rapidly, and not above 20 cc., two-thirds of an ounce, of water was used. This was tested for ammonia at once with the Nessler solution. The second washing produced no appearance of ammonia, done immediately. Ammonia could be observed after an hour and a half's exposure at any rate, but I do not know the shortest period. The results of the washings were as follows. They are the average of 34 experiments for some, and 17 for others, in all 238 experiments.

	Height from floor.		Ammonia.	Height from floor.		Ammonia.
	Ft.	In.	Mgms.	Ft.	In.	Mgms.
Front laboratory	7	3	·013	4	2	·019
2nd landing	6	0	·032			
Balance room	5	1	·015	0	8	·009
1st landing	4	10	·007			
Back laboratory	4	5	·010	0	6	·010
Entrance lobby	6	5	·007			
Office	4	7	·003			
Back yard	4	8	·036	0	7	·042
Back closet	2	3	·105			
Midden			·572			

The first three belonging to the working laboratory are not very regular, as we might suppose, but they never rise very high, nor do they sink to the lowest. The rest, except the second, keep a remarkable similarity, and the differences are very great. In the second there is a disturbance caused by sweeping the floors. On the other days it was requested that everything should be kept still. This of course brings in a practical difficulty and limits the use of the test to cases where care can be used and thoughtful observation, since there are many ways by which dust may be made to interfere, even although the act of sweeping should not take place. The house experiments gave similar gradations.

The result seems to be that a piece of glass, of a definite size, hung up in any place, will receive deposits of ammonia, or substances containing ammonia, in a short time, and by washing the ammonia off with pure water, and testing it with a Nessler solution, it may be seen whether there is too much or not. It is the simplest test for ammonia yet found. Its discoverer deserves great thanks. It must not be forgotten that we may have ammonia in very different conditions; it may be pure, or it may be connected with organic matter. This mode of enquiry is better suited as a negative test to show that ammonia is absent, than to show what is present. When ammonia is present there may be decomposing matter, when absent there is not. I am hoping to make this a ready popular test for air—a test for sewer gases*, for over-crowding, for cleanliness of habitations, and even of furniture—as well as for smoke and all the sources of ammonia. Of course it must be used with consideration, and the conclusions must not be drawn by an ignorant person.

How far it may be used as a test of climate is a matter to be considered also.

After this I made another series of trials with air, Nesslerising the washings at once and not after laborious distillings, as in former cases; the results are very valuable, showing that we obtain comparative quantities equally in this way.

The amount of ammonia obtained in this ready way does not give exactly the same results as the more laborious methods which I have used, but it may be taken as the most convenient. It must be observed that the amount rises exactly where you might expect more organic matter to exist. The lowest is from Prince's-road, outside the town and almost a half a mile from the extreme of the Manchester houses. The next is obtained from an empty yard behind my laboratory, but it is still pure because there was wind and rain, and any one who observes how unusually pleasant it is to breathe the air even of a smoky town during rain and wind will not be suprised. I have not yet, however, had the purest air. I shall require to make a campaign on the moors, hills, and seas before I can give numbers for this. I have not even obtained the best given on land at a distance from manufactures. All this will be done in time.*

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* For sewage also to a larger extent than it has yet been used.

In my office the amount is larger than outside, but the air is not so bad as it is in front and not so good as sometimes in the front where it is open. From the back of the laboratory during fog the ammonia was much higher, but during one day it was excessive, and a special examination of it was made in several streets. The highest amount was obtained at the front of the Cathedral, about mid-day, on the 8th of February, 1878, when the amount was 1·25, or 14½ times more than it had been found in Prince's-road, showing a considerable range.

	Mgms. of Ammonia per cubic metre of air.
Prince's-road	·086
Open yard during rain	·119 and ·102
Front of laboratory	·167 ordinary.
Office	·167
Front and back during fog	·476
Close shut-up room	·413
Closet outside	·800 to ·900
Densest part of fog	1·25

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* Since the paper was read I examined trees and stones at Skelmorlie and Wemyss Bay, finding very little ammonia.

"On the Origin of Elementary Substances, and on some New Relations of their Atomic Weights," by **Henry Wilde**, Esq.

The hypothesis, that the solar system, as at present constituted, was formed by the successive condensations of a gaseous substance rotating under the influence of a central force, has so much evidence in its favour, that it may be affirmed to equal the best of that obtained from the geological record of the changes, which, in past times, have taken place on the surface of the terrestrial globe. That this gaseous or primordial substance consisted of a chaotic mixture of the 65 elements known to chemists is a notion too absurd to be entertained by any one possessing the faculty of philosophic thinking, as the regular gradation of properties observable in certain groups of elements, clearly shows that elementary species are not eternal, but have a history which it is the proper object of physical science to unfold.

One of the principal facts which to my mind establishes the nebular theory of the formation of planetary systems on a firm basis, is Bode's empirical law of the distances of the members of the solar system from each other, and from the central body, as in this law is comprehended the idea of nebular condensation in definite proportions. Now, if elementary species were created from a homogeneous substance, possessing a capacity for change in definite proportions, it is probable that the greater number of elements would be formed during, or after, the transition of the nebular matter from the annular to the spheroidal form. Moreover, as great cosmic transitions are not made *per saltum*, it might be expected that some modification of the law of nebular condensation into planetary systems, as exhibited in Bode's law, would be found on the further condensation of the primitive matter into elementary species.

That relations such as I have indicated exist between the nebular and elementary condensations, represented by the planetary distances on the one hand, with the atomic weights of well defined groups of elementary substances on the other, will be evident on comparing the numbers in the following tables:—

I.

0 . 0 . 4 = 4 Mercury.
1 × 3 + 4 = 7 Venus.
2 × 3 + 4 = 10 Earth.
4 × 3 + 4 = 16 Mars.
8 × 3 + 4 = 28 Ceres, Pallas, &c.
16 × 3 + 4 = 52 Jupiter.
32 × 3 + 4 = 100 Saturn.
64 × 3 + 4 = 196 Uranus.

In the above table the numbers expressing the relative distances of the planetary bodies from the sun, and from each other, are obtained by multiplying successively the difference (3) between the distance of the first and second members of the system, by a geometric series, and adding to the products, the constant distance (4) of the first member from the sun. Now, if the atomic weight of the second member of the alkaline and silver group of metals (Na=23) be multiplied successively by an arithmetical series, then will the products, minus the atomic weight of the first member (Li=7), be the atomic weights of all the elements belonging to that group.

II.

0 . 0 . 7 = Li = 7
1 × 23 . 0 = Na = 23
2 × 23 – 7 = Ka = 39
3 × 23 – 7 = Cu = 62
4 × 23 – 7 = Rb = 85
5 × 23 – 7 = Ag = 108
6 × 23 – 7 = Cs = 131
7 × 23 – 7 = — = 154
8 × 23 – 7 = — = 177
9 × 23 – 7 = Hg = 200

Again, by multiplying in like manner the atomic weight of the second member of the alkaline earth and cadmium group of metals, the products, minus the atomic weight of the first member (Gl=8), are the atomic weights of all the elements of this group.

III.

0 . 0 . 8 = Gl = 8
1 × 24 . 0 = Mg = 24
2 × 24 − 8 = Ca = 40
3 × 24 − 8 = Zn = 64
4 × 24 − 8 = Sr = 88
5 × 24 − 8 − Cd = 112
6 × 24 − 8 = Ba = 136
7 × 24 − 8 = — = 160
8 × 24 − 8 = — = 184
9 × 24 − 8 = Pb = 208

The further relations observable between interplanetary voids and atomic condensations of the natural groups of elements in tables II. III., are as follows:—

1. The regular geometric series of the planetary distances commences at the second member of the system, and the regular arithmetical series of atomic weights commences at the second and corresponding member of each group.

2. As the atomic weight of the second element in each group is half the sum of the atomic weights of the first and third elements, so is the distance of the second member of the solar system an arithmetical mean, or half the sum of the distances of the first and third members.

3. The atomic weight of the fourth member in each group of elements is equal to the sum of the atomic weights of the second and the third; and the distance of the fourth member of the solar system is also equal, within a unit, to the sum of the distances of the second and third members.

4. As the smallest planetary distance is a constant function of the distances of the outer planetary bodies, so is the smallest atomic weight in each group a similar function of all the higher members of the series to which it belongs. It will also be observed that the plus and minus signs of

these constants are correlated respectively with the interplanetary spaces, and the elementary condensations.

5. Each of the atomic weights, after the third in the groups, is an arithmetical mean of any pair of atomic weights at the same distance above and below it; and the distance of each member of the solar system (minus the constant 4) is a mean proportional of the distances of any two members, externally and internally to it, from the central body.

6. The geometric ratio of the planetary distances from each other terminates at the two members nearest the central body, and approaches to an arithmetical one; and a similar departure is also noticeable from the regular arithmetical series of the atomic weights of the first two members of the groups, which renders the third less than an arithmetical mean of the atomic weights of the second and fourth members.

While most of the atomic weights in tables II. III., excluding fractions, agree with those generally received by chemists, the remainder, except Cæsium = 133, do not vary more than a unit from the classical numbers. When it is considered that some of these numbers have been obtained by doubling the fractions of the old atomic weights, and that slight differences in the determinations may arise from the latent affinity which some elements have for minute quantities of another, the numbers in the tables are remarkably near to those determined by experiment—more so, in fact, than is Bode's law to the actual distances of the planets from the sun.

It will be observed that there are gaps to be occupied by two elements in the first group, with atomic weights 154 and 177, and by their homologues of position in the second group, with atomic weights 160 and 184, which remain to be discovered.

The numerical relations subsisting among the atomic weights in tables II. III., and their resemblance to the homologous series in organic chemistry, afford further evidence in support of the theory that elementary species are formed by the successive condensations of a primordial substance of small specific gravity and low atomic weight. The physical and chemical properties of hydrogen, especially its low atomicity and its exact multiple relations with many elementary substances, long since suggested to Prout that this element might be the ponderable base of all the others.* Prout's hypothesis has not, however, made much progress, as chemical knowledge was not sufficiently advanced in his time to enable the intermediate steps to be perceived by which elements of high atomicity could be built up from hydrogen; and, besides this, the hypothesis afforded no explanation of the widely diverging properties of elements having nearly the same atomic weights. If, however, it be assumed that a particle of hydrogen combines successively with one, two, three, or more of its own particles, to form the molecules H2, H3, H4, H5, H6, H7, and that each of these molecules forms the type of a group of elements under it, the intermediate steps between the low atomic weight of hydrogen, and the high atomic weights of other elements are perceived, and the different properties of elements of approximately equal atomic weights admit of a rational explanation.

Although it is herein assumed that hydrogen is the ponderable base of all elementary species, it is probable that this element itself, as further maintained by Prout, may have been evolved from an ethereal substance of much greater tenuity.† Further knowledge of the outer regions of the solar atmosphere, and of the zodiacal light, may possibly indicate the steps by which hydrogen was formed.

I would also observe that the term "molecule" is here used only in the sense of a larger or denser particle of matter, and does not imply the idea of a composite aggregation of the separate particles, each preserving its distinctive character after the molecule is formed, any more than rain drops preserve their distinctive character after falling into the ocean. It appears to me much more in accordance with the truth of nature to suppose that the smallest conceivable particle of a chemical substance or compound, has the same physical properties absolutely as the mass. If it be objected that such a union of particles would have relations of infinity, and is therefore inconceivable, it may be answered that the central particles of a rotating body have mathematical and physical relations of a similar kind, and as the instrument of thought is incapable of forming a distinct conception of the magnitude of the infinitesimals involved in a centre of rotation, still less is it capable of comprehending the mode of union of the unknowable essences on which the physical qualities of chemical substances, after combination, depend. Philosophical chemists, I apprehend, will hereafter be able to refer the origin of the theory of the composite structure of matter, after chemical union, to the influence of ideas derived principally from the mechanical mixtures employed in pharmacy, and in the culinary art.

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* *Annals of Philosophy*, vol. vi., 330, 1815; vol. vii., 113, 1816.

† Prout's Chemistry and Meteorology, 8th Bridgewater Treatise, p. 130.

In the present hypothesis, it is assumed that a mass of hydrogen, of a curvilinear form, acquired a motion of rotation about a central point, which caused it to take a spiral or convoluted form. As each successive spiral or convolution was formed, the particles of hydrogen combined with themselves as far as the septenary combination, to constitute the type of each group of elements—the number of types or groups being equal to the number of convolutions of the rotating gas. According to this view, the elementary groups my be represented as forms of Hn, $H2n$, $H3n$, $H4n$, $H5n$, $H6n$, $H7n$; the internal convolutions forming the highest type $H7n$, and the outer convolution the type Hn.

That on a further condensation of the elementary matter, a transition from the spiral to the annular form occurred, during, or after which, the group or species under each type was generated in concentric zones, and in the order of their atomic weights, until the highest member of each species was formed. That as the elementary vapours began to condense, or assume the liquid form, their regular stratification would be disturbed by eruptions of the imprisoned vapours from the interior of the rotating mass. This disturbance would be further augmented by the subsequent combination of the negative with the positive elements, and also by the variable solubility of their newly formed compounds; so that the evidence of such stratification of the elementary vapours as I have indicated, must necessarily be more fragmentary than that of the geological record. The constant association in nature, however, of several elements belonging to the same group, a remarkable example of which is the presence of lithium, potassium, rubidium, and cæsium in a single mineral, *lepidolite*, appears to confirm this view of the primitive arrangement of elementary vapours.

In the annexed table are arranged all the known elements in natural groups, wherein gaps appear as in tables II. and III., which indicate the existence of missing elements. The atomic weights of other elements which have not been sufficiently investigated are also determined.

If the theory which I have enunciated of the evolution of elementary substances from hydrogen in definite proportions be correct, the numbers representing the atomic weights, also represent the number of particles of hydrogen from which the elements were formed. Where these numbers do not coincide exactly, as in the case of Cu=62, and its homologue of position, Zn=64, which are each a unit less than the classical numbers, it is not to be supposed that these discrepancies are due to errors of experiment, but to some unknown cause which prevents their true atomicity from being ascertained.

Although the ideas of chemists on the classification and quantivalence of elements have greatly changed during recent years, there is no question that the alkaline metals, lithium, sodium, potassium, rubidium, and cæsium, belong to the group which I have classified under H*n*. Chemists are also agreed that silver, notwithstanding the great divergence of some of its characteristics from those of the alkaline metals, also belongs to the same group. Now some of the physical and chemical properties of copper and mercury are more nearly allied to those of silver than to metals of other groups, and recent investigations have shown that silver may, like copper, be regarded as bivalent, since many of its compounds can be represented by formulæ exactly analogous to those of cuprous compounds with which they are isomorphous.* The position of Hg, Ag, and Cu, as alternate members of the series H*n*, indicate their relationship with sodium, and are thereby brought into still closer connexion with Li, K, Rb, and Cs. That a relationship exists between sodium and silver, by the isormorphism of their anhydrous sulphates, and in other ways, has already been pointed out by Odling. The greater specific gravity of sodium, while possessing a lower atomic weight than potassium; its passivity in the liquid state to the action of chlorine;† and its inferior volatility and oxidability to K, confirm the relationship of Na to the heavy metals of the series.

From what cause elements possessing physical properties so widely different, should be associated alternately in regular order in the same series, can only, in the present state of knowledge, be a subject of speculation; but, if the views which I have enunciated on the formation of the types H*n*—H7*n* be correct, it may be conceived that after the transition of the cosmical vapours from the spiral to the annular form, the gaseous material of each pair of members might rotate in concentric zones, separate from each other by an interval of space. It may be further conceived, that the rotating zones of elementary matter were of sufficient thickness to cause a difference of density between their upper and lower regions. That the zones were in a highly electrical condition, and that their mutual influence on each other, through the annular space between them, would induce opposite electrical conditions in their external and internal regions; all the inner and denser regions of the zones being in a negative, and the outer or rarer regions in a positive electrical condition. Each zone would then be in a condition to form an electro-positive, and an electro-negative element, which, on a subsequent condensation, would separate and form two zones of elements having dissimilar properties alternating with the other members of the same series.

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* Quantivalence of Silver,—Wislicenus, Watts, Dic. Chem., 2nd Suppl., 1088.

† Watts, Dic. Chem., Suppl., 1030.

Just as silver and sodium are the connecting links between Hg and Cu, and the alkaline metals Li, K, Rb, and Cs; so do cadmium and magnesium connect lead and zinc with the alkaline earth metals glucinum, calcium, strontium, and barium, which I have classified as forms of $H2n$. The classification of glucinum with the alkaline earth metals has only recently been made, but chemists are not yet agreed upon the atomic weight of this element, as it has been fixed at Gl=7 (Awdejew) and Gl=9·4 (Reynolds). It may, however, be suspected from the anomalously high specific gravity assigned to glucinum (2·10) as compared with that of magnesium (sp. g. 1·74), and with their homologues of position Li. sp. g. 0·59, and Na. sp. g. 0·97, that this element has not yet been isolated in a state of purity.* By assigning to glucinum the atomic weight Gl=8, it enters as a multiple into all the members of the series H2*n*, and may be regarded as the product of the first, second, or third powers of H2.

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* Since this paper was written, MM. Nilson and Petterson have communicated to the French Academy the results of their researches on the physical properties of glucinum, and have found for the metal a density equal to 1·64, which, although still too high, the theoretical density being about 1·3, is less than that of magnesium, and, consequently, stands in the same order of density as lithium and sodium.—*Comptes Rendus*, April 1st, 1877, p. 825.

While the property of quantivalence would appear to be correlated with the number of hydrogen particles in the typical molecules from which the elements were evolved and is a valuable aid in the classification of elementary species, this property in the present state of knowledge is not in many cases sufficient, of itself, to indicate the group to which an element belongs. This will be seen from the recognised bivalency of copper and mercury, and by the doubtful quantivalence of silver, and by analogy of sodium, all of which belong to the series H*n*. That tetratomic lead=208, is a member of the group H2*n*, is shown by the isomorphism of its oxide, carbonate, and sulphate, with the oxides, carbonates, and sulphates of barium, strontium, and calcium, besides which there is no other place vacant in the system of elements where one with the atomic weight and physical properties of lead would fit.

Were it not for the analogous physical properties, and the numerical relations subsisting among the elements grouped as forms of H3*n*, their classification from the property of quantivalence alone would have hardly been possible. There can, however, be little doubt that aluminum, yttrium, erbium, and thorium are rightly classified together, and that indium and thallium are true analogues of each other. As considerable interest attaches to this group at the present time, on account of the recent additions which have been made to it by the aid of spectral analysis, I here show the atomicities of its members in a separate table, calculated on the same principle as those in tables II. III.

IV.

0 . 0 . 12 = C = 12
1 × 27 . 0 = Al = 27
2 × 27 − 12 = — = 42
3 × 27 − 12 = — = 69
4 × 27 − 12 = — = 96
5 × 27 − 12 = Yt = 123
6 × 27 − 12 = In = 150
7 × 27 − 12 = E = 177
8 × 27 − 12 = Tl = 204
9 × 27 − 12 = Th = 231

It will be observed that there are three elements missing in this group, the atomic weights of which can be predicted in like manner with those of the missing elements in the preceding groups. The table also affords the means of correcting and determining the atomicities of elements of the series which, from their rarity, have not been sufficiently investigated. It will be further observed that, besides the similar numerical relations of the members of this group with those shown in tables II. III., the atomic weights are all multiples of 3, and are classified accordingly as forms of H3*n*.

The spectral reactions of this series of elements are remarkable from the oxides of carbon, and erbium, giving a spectrum of lines at low temperatures, and by the simplicity of the spectral lines of indium and thallium in the more refrangible parts of the spectrum. The atomic weights of C, Al, Tl, and Th, are indentical with those generally received, and afford presumptive evidence that the atomic weights of the intermediate members are equally correct. It will, however, be observed that the atomic weights of yttrium and indium are double the accepted numbers (Y = 61·7, In = 75·6); but in regard to the latter element, it has not yet been definitely agreed which multiple of 37·6, the original determination, shall be the classical one, as the atomicity has been fixed by different chemists at 75·6, 113, and 150, the number assigned to it in the table. The relations which the double atomic weights of In, and Yt, have to each other, and with their homologues of position Cs, Ba, and Ag, Cd, in tables II. III., render it highly probable that the atomic weights of Y and In, in the table are correct. For similar reasons it is probable that the atomic weight of erbium will be found to be 177. It is only very recently that any investigations of the atomic weight of this rare element have been made, from the difficulty attending its isolation from yttrium, with which it is found associated in nature. According to some chemists, the atomic weight of erbium is 112·6, which, in relation to 177, is nearly in the ratio of 5 to 8. The more recent researches of M. Cleve, on the quantivalence of this element have, however, raised its atomic weight to 170·55* which, considering the wide difference between it and the previous determination, is a near approximation to the number in the table. The researches of the same chemist have also raised the atomic weight of yttrium from 61·7, the accepted determination, to 89·5, or three-fourths the calculated value. Now the history of chemical science abundantly shows that it is only after long and repeated investigation that the highest quantivalence of an element can be ascertained, and the result of M. Cleve's researches is a further confirmation of the correctness of the atomic weights of yttrium and erbium given in the table.

By comparing the electro-positive members of the series H*n* with those of H2*n*, it will be seen that a complete parallelism exists between them; the light alkaline, and alkaline earth metals alternating with the heavy members in homologous positions in both series. Odling has already indicated that this is the natural order of the dissimilar members of the zinco-calcic group of elements,† and similar alternations in other natural groups have been recognised in the arrangement of elements proposed by Mr. Newlands‡ and Mendeleeff.||

Just as Cu = 62, Ag = 108, and $x = 154$, alternate with Rb = 85, Cs = 131, and $x = 177$, in the series H*n*; and Zn = 64, Cd = 112, and $x = 160$, alternate with Sr = 88, Ba = 136, and $x = 184$; so in the series H3*n*, do $x = 69$, Yt = 123, and Eb = 177, alternate with $x = 96$, In = 150, and Tl = 204.

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* Bull, Société Chemique, Paris, tome xxi., 344, 1874.

† Watts, Dic. Chem., 1865, vol. iii., 963—Classification of Metals.

‡ Chem. News, vol. xii., 83; vol. xiii., 113.

|| Die periodische Gesetzmäfsigkeit der chemischen elemente—Ann. Chem. Pharm.; Suppl. Band, viii., 133—229, 1872; Phil. Mag. 5th S., vol. i., 543.

Again, just as K, Rb, Cs, and $x = 154$, are analogues of each other in the series Hn, so are $x = 42$, $x = 96$, In, and Tl, analogues of each other in the series H3n, and are in homologous positions with the alkaline, and alkaline earth metals in the series Hn, and H2n. The specific gravities of analogous members of these two series, except glucinum, which is anomalous, increase in the order of their atomic weight, and so far as the specific gravities of the members of the series H3n, have been ascertained, they follow the same order. Now M. Lecoq de Boisbaudran has shown that the new metal which he has discovered, and named gallium,* is, from its spectral reactions and other properties, the analogue of indium and thallium. The position of the new metal in the series H3n, should therefore be either – $= 42$, homologous with Ca, and K, or – $= 96$, homologous with Sr, and Rb. In comparing the alkaline metals of the series Hn, the specific gravity of sodium (0·97) as will be seen, is greater than that of potassium (0·86), although Na has a less atomic weight; and the same inversion of specific gravities in relation to atomic weights is observable in their homologues of position Mg., sp. g. 1·74, and Ca, sp. g. 1·58, in the series H2n. It may therefore be assumed that the missing member $x = 42$, H3n, would have a less specific gravity than Al, sp. g. 2·56; probably 2·5. Now, the specific gravity of gallium as determined by M. Lecoq de Boisbaudran is 5·9;† and its analogues indium and thallium have specific gravities of 7·42, and 11·9 respectively, consequently $x = 42$, is not gallium. If gallium were $x = 69$ it would be the analogue of Y, E, and Th, and homologous in position with Zn and Cu, whereas it has been shown to be the analogue of In, and Tl, and homologous in position with Sr and Rb. There is then no other place for a metal having the physical properties of gallium but the one assigned to it in the series H3n, with the atomic weight = 96, and forming a triad with indium and thallium. If, however, the experimental determination of the atomicity of gallium pass through the same stages as the atomicities of indium, yttrium, and other members of the series, its atomic weight will be represented by the submultiple and proportional numbers 48 and 72.*

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* Comptes Rendus, tome lxxxi., 403, 1000, 1865.

† Phil, Mag. 5th S., vol. ii., 398.

Just as silver and copper are analogues of each other, and are frequently associated in nature; and, just as their homologues, cadmium and zinc are analogues, and are also found together, so is yttrium the analogue of $x = 69$, and will be found associated with it in nature. Now, if $x = 69$ be not the terbium of Mosander and Delafontaine, and the researches of Bahr and Bunsen render the existence of this element doubtful, it is probable that $x = 69$ is cerium, as this element and yttrium are nearly always found associated in the mineral species cerite and yttrocerite. Moreover, it will be observed that $x = 69$ is just 1·5, or 0·75 the atomic weight of cerium, according as it is regarded as 46 or 92. Mendeleeff and other chemists have already proposed 138 as the atomic weight of cerium,† which is double that of $x = 69$. MM. Hildebrand and Norton have recently obtained cerium, lanthanum, and didymium in a massive state, and have thereby been able to investigate some of the physical properties of these rare metals.‡ According to these experimenters the specific gravities of Ce, La, and Di, range between 6 and 6·7. Bearing in mind that elements of approximately the same atomic weights and specific gravities generally belong to different series, and that the specific gravities of analogous members in each series increase in the order of their atomic weights, it would appear that cerium does not belong to the same series as lanthanum and didymium. Moreover, considering the important position which $x = 69$ occupies in relation to its analogues Al, Y, and the position which these three elements occupy in relation to their homologues Mg, Zn, Cd, and Na, Cu, and Ag, it may be doubted if $x = 69$ should, up to the present time, have remained undiscovered, especially as all its analogues of the series Th, E, Y, and Al, are well known. If, therefore, $x = 69$ be cerium, the only element missing in the series H3*n* is $x = 42$, the analogue of Ga. In, and Tl. As these elements have been discovered by spectrum analysis, it is probable that $x = 42$ will also be found by the same means. It may however be observed, that the characteristic lines of the alkaline metals in the series H*n*, and of their homologues H3*n* advance in the blue or violet end of the spectrum, towards the more refrangible parts in the inverse order of their atomic weights. The spectral lines of $x = 42$ must therefore be sought for in the violet or ultra violet part of the spectrum. The high refrangibility of the lines which the missing element will have, may be the reason why it has hitherto escaped detection, as from the wide distribution in nature of its homologues of position Ca, and K, in relation to their respective analogues Sr, and Rb; $x = 42$ ought to be more abundant in nature than gallium.

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* From a calcination of the gallo-ammoniacal alum, M. Lecoq de Boisbaudran has recently found for gallium the equivalent 70·03, and from a calcination of the nitrate, 69·6—*Comptes Rendus*, April 15th, 1878. The researches of M. Berthelot on the specific heat of gallium indicate, however, a higher equivalent for the metal than 70·03, as the atomic heat calculated from this determination (5·52 solid) is lower than that of any other metal except silicium.—*Ibid*, April 15th, 1878.

† Ann. Chem. Pharm. Suppl. viii., 185—190.

‡ Chem. Soc. Journal, 1876, vol. ii., 276.

From the physical and chemical relations which subsist among the halogens Fl, Cl, Br, I, and the alkaline metals Li, Na, K. Rb, Cs, chemists have already justly considered these elements as positive and negative analogues of each other and of hydrogen. In accordance with this view, I have classified the halogens as negative forms of the series H*n*. By assigning to these elements the positions shown in the table, it will be seen that besides the triad of atomic weights formed by Cl, Br, and I, there is a common difference of 4 between the atomic weights of the halogens and their positive homologues of position Na, K, Rb, and Ca. Now if the groups of oxygen elements O, S, Se, Te, be considered as negative forms of H2*n*, homologous in character and position with the negative forms of H*n*, it will be seen that besides the triad of atomic weight formed by S, Se, and Te, there is a common difference of 8 between them and their positive homologues Mg, Ca, Sr, and Ba; or double the common difference between the positive and negative members of the series H*n*. The oxygen elements are multiples of 2, 4, 8, and 16, and may accordingly be considered as products of the first, second, third or fourth power of H2*n*. Whichever view be taken of the formation of the first negative member of the series H2*n*, it is probable that both fluorine and oxygen were not formed direct from H*n*, and H2*n*, but from members homologous in position with Li, and Gl, but which have become extinct by absorption into F and O.

Another numerical relation subsisting among the halogens which it may be of interest to point out is, that the difference of a unit in their atomic weights will make them multiples of 3 and 9, and these numbers, commencing with Cl = 36, are all respectively three times the atomic weights of the first three members of the series H3*n*. These relations would indicate that the halogens, usually regarded as monatomic, are also built up in multiple proportions, and may also throw some light on the variable quantivalence which Wanklyn and other chemists have shown the alkaline metals and halogens to possess.

The recent researches of chemists leave no doubt that all the elements which I have classified as forms of H5*n*, except boron, belong to the same group. Now, boron bears a greater resemblance to phosphorus in its combinations and occurrence in nature than it does to other elements, and whether the first three members of the series be considered as forms of H5n, or H5$n+1$, they form a triad as well defined as their homologues of position in H3n, H2n and Hn. Triads are also formed by antimony, arsenic, and phosphorus:—bismuth, antimony, and phosphorus:—tantalum, niobium, and boron:—$x=140$, As$=75$ and B$=10$:—$x=140$, Nb$=95$, and V$=50$. The atomic weights of boron, phosphorus, and vanadium have been so carefully determined by chemists, as to preclude any doubt of their being represented by H5$n+1$, rather than H5n; but the fact that arsenic, antimony, and bismuth are better represented by the formula H5n, and that Cu, and *Zn*, in the series Hn, and H2n, exhibit the same constant minus difference from the classical atomic numbers as B, P, and V, are further indications of some unknown property of the elements which conceals their exact multiple relations from view. If the discovery of two new elements of this group by Hermann,* to which this chemist has given the names of neptunium, and illmenium be confirmed, the former element will have an atomic weight of 140, and the latter element an atomic weight of 165, as shown in the table.

Although the numerical relations of the members of the series H5n are very interesting, yet, it will be seen that the ratios are not so simple as those of the series Hn, H2n, H3n, as multiples of the second member, minus the first, do not give the atomic weights of the other members of the series.

The series H4n, besides being incomplete by reason of the absence of several of its members, the atomicity of lanthanum and didymium is not yet agreed upon by chemists. There can, however, be no question as to the position of titanium as the third member of this series, as there is no other place vacant where an element with an atomic weight of 48 would fit, while the isomorphism of rutile with cassiterite and zirconia indicate the relation of tin and zirconium with the same series.

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* *Nature*, April 12th, 1877. *H. Kolbe's Journal für praktische chemie*, Feb. 1877, p. 105—150.

The classification of uranium presents some difficulty on account of the fewness of its analogies with other elements, but there can be little doubt that the atomic weight assigned to U = 120, until recently, is much too small; as there are no elements with atomic weights so low, correlated with specific gravities so high as that of U = 18·3. From a study of the chemical combinations of this element, Mendeleef has assigned to it the atomic weight of 240,* or double the number formerly received, and which number I have adopted. The admission of this high atomic weight, however, separates uranium from chromium, molybdenum and tungsten with which it has been classified, as there are no elements of approximately the same high specific gravities as tungsten = 18·26, and uranium = 18·3, correlated with so great a difference of atomic weights as U = 240, and W = 184. From the fact that the highest places in all the series, except that in $H4n$, are filled up with their highest members, and that uranium is generally found in combination with the mineral species *yttrotantalite*, *fergusonite*, *polykrase*, *pyrochlore*, *pyrrhite*, containing elements of the series $H3n$ on the one side, and in combination with minerals containing elements of the series $H5n$ on the other, I have classified uranium as the highest form of $H4n$. The two lower forms of $H4n$, as will be seen from the table, are missing; but, assuming that titanium is the highest member in a triad with the missing elements, the atomic weights of the latter are 16 and 32, isomeric with oxygen and sulphur. It may, however, be surmised that no elements now exist to fill the gaps in the series, as they may have become extinct by absorption into titanium and its analogues, or by transformation into the negative forms of $H2n$.

The elements which I have classified as forms of $H6n$ are only three in number, and the atomic weight of chromium = 52·2 establishes its position as the third member of the series, and there is no other place for an element with the chemical and physical properties of chromium vacant in the table. For like reasons the positions in the series of molydenum and tungsten (the analogues of chromium) are also determined. By assigning to chromium the constitution 9 H6, it forms a triad with the missing elements $x=36$, and $x=18$, which are within a unit, the atomic weights of fluorine and chlorine.

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* Ann. Chem. Pharm. Suppl., viii., 178—184.

In the arrangement of the elements which I have classified as H7*n*, little assistance is derived from known analogies, when nitrogen and silicium are admitted in the same series with the iron and platinum groups of metals; yet, it might be expected that elements so abundant, and so widely diffused in nature as nitrogen, silicium, and iron, would occupy important positions in any rational classification of elementary species. We have seen that the first three places in the preceding series H*n*, H2*n*, H3*n*, H5*n*, are all occupied by elements with atomic weights which exclude nitrogen, silicium, and iron, while the latter element is excluded from the series H4*n*, and H6*n*, by chromium and titanium. The atomic weight of N, Si, and Fe, besides being whole numbers, are exact multiples of 7. N, and Si, are, consequently, excluded from the vacant homologous positions in the series H4*n*, H6*n*.

Since the investigation of the properties of silicium by Berzelius, who regarded silicic acid as a trioxide, much discussion has arisen as to whether the atomic weight of silicium be 21 or 28; or the formula for its oxide SiO_3, or SiO_2. Chemists are now generally agreed upon the latter formula for silicic acid, and have accordingly classified silicium with titanium, as the oxide SiO_2, agrees with titanic acid TiO_2. Now, if silicium were the true analogue of titanium, the oxides of these elements should be isomorphous, whereas the crystalline form of quartz is hexagonal, while rutile, anatase, brookite, zirconia and tinstone (similar oxides of members of the series H4*n*), are tetragonal; consequently, silicium does not belong to the series H4*n*.

By assigning to silicium the atomic weight 35, it forms with nitrogen and iron a triad similar to the first three members of H*n*, H2*n*, H3*n*, H5*n*. The position of Si = 35 as the second member of the series H7*n*, not only throws new light on the disputed atomicity of this element, but also explains the anomalous atomic heat which has been assigned to it.

Through the classical researches of Regnault the specific heat of silicium was found to be 0·176.* The determination was made with specimens of the metal of considerable size, and in a state of compactness and purity, to receive a polish which formed a perfect mirror. The above number multiplied by 28, the highest atomic weight assigned to Si, gives the product 4·93, while the law of Dulong and Petit, requires the value 6·25.

In discussing the cause of the anomalous atomic heat of silicium, Regnault pointed out that in order that it might enter into the law of the specific heat of other elements, it would be necessary to write the formula of silicic acid Si_2O_5; it would then resemble that of nitric, phosphoric, and arsenic acids. The atomic weight of silicium would then be 35, and the product of this number and the specific heat would be nearly 6·25, which agrees with the analogous products which other simple bodies give. By assigning to silicium a higher atomic weight and a polybasic character like that of phosphorus or nitrogen, Regnault remarked that it is easy to explain the existence of the great number of silicates which nature presents in well defined and beautiful crystals, and to understand the existence of the natural hydro-silicates.

Whichever view chemists may ultimately adopt in regard to the constitution of silicic acid, or whether its atomic weight be fixed at 3H7, 4H7, or 5H7, silicium will still retain its positions as the second member of the series H7*n*. The chief properties which distinguish the elements of the series H7*n* are their high fusing point; their occlusive affinity for hydrogen; and their passivity in the presence of ordinary reagents, to which iron, under peculiar conditions, forms no exception. In regard to their occlusive affinity for hydrogen; the relation of nitrogen to iron and palladium may explain the existence of the ammonium amalgam, in which nitrogen and hydrogen are held together in the nascent state by means of mercury. The formation of silicium hydride by electrolysis, in a manner analogous to that of the ammonium amalgam, would also indicate for silicium a similar occlusive affinity for hydrogen to that possessed by nitrogen.

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* Annales de Chimie et de Physique, tome lxiii., 24-31, 1861.

Although gold in some recent classifications of elements has been separated from the platinum metals, yet, in its primary qualities, it exhibits closer analogies with them than with the members of any other series, and there is no other place vacant in the groups which an element with the atomic weight and physical properties of gold would fit. The constant association in nature of quartz, hematite and specular iron ores, with gold and platinum is a fact fully recognised by chemical geologists,* and confirms the positions assigned for Si, Fe, and Au, in the table as forms of H7*n*.

The remarkable resemblance which the members of the iron group have to one another, while their atomic weights are nearly if not exactly the same, has long been a subject of much interest to philosophical chemists, and if the views which I have enounced respecting the formation of elementary species by condensation be correct, the cause of these resemblances admit of a possible explanation. From the great abundance and wide distribution of iron in nature, it is probable that the vapour of this element would form a zone of considerable depth; the upper and lower regions of which, by differences of pressure and temperature might produce allotropic varieties before a definite change to the next higher members in the series occurred. When once varieties of an element were formed, these varieties would be propagated through successive condensations into the next higher members of the series, just as they are found in the palladium and platinum groups of metals. Chemists have already observed that each of the metals of the palladium group appears to be more especially correlated with some particular member of the platinum group, and all are found associated together naturally in the metallic state. If the four members of the platinum group be considered the analogues of the corresponding members of the iron and palladium groups, it will be seen that one of the members of the latter group is missing. M. Sergius Kern, a Russian chemist, has recently discovered a new metal which he classifies with the platinum group, and has given to it the name of davyum.* The specific gravity of the new metal was found to be 9·39, and preliminary experiments on its equivalent show that it is greater than 100 and supposed to be 150—154. Now the specific gravity and atomic weight of the new metal exclude it from the platinum group, and also from the iron group of metals; davyum is therefore the missing element in the palladium group, and will have a specific gravity of about 11, and an atomic weight of 105; or the same density and equivalent as the other members of the group. The state of aggregation of the small quantity of the new metal obtained by M. Kern, may have prevented the same specific gravity being found for it as for the other members.

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* *Bishcoff's Chemical and Physical Geology.* Vol. iii., 534. Cavendish Soc. Works. *Murchison's Siluria.* Chap. xvii., 433-439.

Although I have designated the highest members of the series H7*n*, as the platinum group, yet if the slight differences in their atomic weights and physical properties admit of explanation by the assumption of their being allotropic varieties of each other, then gold, palladium, and iron, may stand at the head of their respective groups, and determine the species to which the varieties belong. It is no objection to the theory of the members of the respective groups being varieties of each other, that they cannot by any known power of analysis be resolved into their primaries, as the same objection would apply to the natural varieties of organic species determined by naturalists.

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* Comptes Rendus, tome lxxxv., 72, 623, 667. 1877.

We have seen that the quantivalence of most of the members of the preceding groups H*n*, H6*n*, is in some way correlated or dependent on the construction of the typical molecules at the head of each series; but in the series H7*n*, the only element which is known to be septivalent is manganese; but the relation which this metal has to the iron group; and bearing in mind that the determination of the highest quantivalence of elements is limited by the knowledge of chemists at particular times, and is only arrived at after much research, the septivalency of manganese indicate a much higher quantivalence for the other members of the series H7*n*, than has up to this time been accorded to them.

I have hesitated to introduce hypothetical elements alternating with the iron, palladium, and platinum groups, as the regular sequence of elementary forms is broken by varieties, and from the density of the typical molecule H7, it may be that the members of this series are limited to those shown in the table. The density of the typical molecule H6*n* may also explain the absence of members alternating with Cr, Mo, and W, and I have therefore only introduced one hypothetical element in this series, the analogue of Cr with the atomic weight = 144.

Considering how nearly the numbers representing the molcular constitution, and atomic weights of the members in homologous positions in the higher groups approximate, the idea occurs that the subsequent condensations of these higher groups are in some way influenced or determined by the antecedent condensations of homologous members of the lower groups, and may be the cause of the departure in the higher groups from the simple ratios and multiple relations observed amongst the elements of the series Hn, and H$2n$. Such perturbations would appear to be similar to those which the planetary bodies exercise on each other to produce modifications in the forms of their orbits, but I leave this question to the further consideration of physicists and astronomers.

The complete parallelism of the halogens and oxygens to each other, and their intensely electro-negative character, point irresistibly to the conclusion that at one period of their history these elements existed in a state of isolation from all the others. How, and under what conditions, they acquired their electro-negative properties can in the present state of knowledge be only a matter of conjecture; but it may be conceived that these elements may have existed originally in the form of a zone or zones revolving within the moon's orbit, but high above the incandescent terrestrial surface; probably before the lunar substance changed from the annular to the globular form. These intra-lunar zones may have gradually acquired their electro negative properties by lunar and terrestrial induction, and by the loss of their primitive heat by radiation into space. Their orbits being too near the earth to permit the zones to assume the spheroidal form, they would upon rupture become incorporated with the positive terrestrial elements, and remain dissociated till the temperature of the mass was sufficiently reduced to enable chemical combination to take place. If Draper's discovery of oxygen in the sun be confirmed, the hypothesis of the existence of an intra-mercurial zone of negative elements which subsequently united with the solar positive elements is at least as probable as the assumption of an intra mercurial planet which has recently been discussed by astronomers. May not the sudden increase in the brightness of variable stars like T Coronæ, Nova Ophiuchi, 1848, and Nova Cygni, 1876, be due to the intense heat generated by the union of zones of negative elements with the central bodies round which they revolve, or by the condensation of lower into higher forms of elementary species ?

All the positive forms of H2*n*, except glucinum and lead, are well ascertained solar elements, and the remarkable relations which the members of this group have to those of H*n* render it highly probable that, besides sodium and copper, other members of H*n* are present in the solar atmosphere. From the fact that aluminum, titanium, chromium, and the irons are solar species, higher forms of these elements may also be expected to be found in the sun.

The numerical relations of the atomic weights to which I have directed attention, and the brief outline of a theory of the origin of elementary species which I have founded upon them, give new force to the doctrine of the transmutable nature of elementary substances. But when the synthetical formation of organic compounds is regarded as the greatest triumph of modern chemical science, the problem of building up the higher elements from the lower, may well be deemed insoluble, as they have been formed under cosmical conditions of which we have little or no acquaintance. Very different, however, is the aspect of the problem of resolving the higher elements of each series into their respective types or into hydrogen. For just as by the application of heat, the higher members of homologous series are resolved, through their lower members, into their ultimates, so may it be expected that the elements themselves will, in their turn, give way to more powerful instruments of analysis.

When it is considered, that through the investigations of Dumas, Cooke, Odling, Mendeleeff and others, nearly all the mathematical relations of the atomic weights to each other have been unfolded during the brief interval of thirty years, so that but few steps are now required to render the natural classification of the elements complete, the resolution of elementary species into their primordial ultimates would not appear to be far off.

	+ **H***n*	—	**H5***n*	**H6***n*	**H7***n*	
1						
2	Li = 7		B = 10	— = 18	N = 14	
3	Na = 23	**F** =	P = 30	— = 36	Si = 35	
4	K = 39	**Cl** =	V = 50	Cr = 54	Fe = 56	56
					Mn = 56	55
					Ni = 56	58
					Co = 56	58
5	Cu = 62		As = 75			
6	Rb = 85	**Br** =	Nb = 95	Mo = 96		
7	Ag = 108		Sb = 120		Pd = 105	106
					Rh = 105	105
					Ru = 105	105
					Da = 105	—
8	Cs = 131	**I** = 1	— = 140	— = 144		
9	— = 154		— = 165			
10	— = 177		Ta = 185	W = 186	Au = 196	196
					Pt = 196	197
					Ir = 196	197
					Os = 196	198
11	Hg = 200		Bi = 210			

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, March 26th, 1878.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

The following gentlemen were elected officers of the Section for the ensuing year:—

President.
JOSEPH BAXENDELL, F.R.A.S.

Vice-Presidents.
E. W. BINNEY, F.R.S., F.G.S.
ALFRED BROTHERS, F.R.A.S.

Treasurer.
JAMES BOTTOMLEY, B.A., D.Sc.

Secretary.
THOMAS MACKERETH, F.R.A.S., F.M.S.

April 23rd, 1878.

E. W. BINNEY, F.R.S., F.G.S., Vice-President of the Section, in the Chair.

"On some Colorimetrical Experiments," by JAMES BOTTOMLEY, B.A., D.Sc.

If a white disc be sunk in a coloured solution, the intensity of the colour perceived will be a function of the depth of the disc and the quantity of colouring matter present. If, in any set of experiments, we make the intensity of colour constant, then the depth of the disc will be a function of the quantity of colouring matter only. Occasionally in chemistry we have to deal with quantities so minute that we can no longer use the balance for their determination, and yet those small quantities when they yield coloured solutions are evidently comparable among themselves. In a short communication which I made to this Society (vol. xv., 63), I suggested that such a process as above indicated might be applied for the measuring of such traces, assuming that the depth of the disc would be inversely as the quantity of colouring matter present. Some experiments were made on colouring matter in solution. Glass cylinders were used about 10in. high and 2in. diameter; to one side of each cylinder was pasted a strip of paper marked with an arbitrary scale (each degree of the scale I afterwards found to be 27mm.), the white surfaces used were crucible lids; a piece of glass rod resting on one of the cylinders, having the string of the moveable disc attached to it, served as a windlass for raising or depressing it. To avoid the use of many ciphers I have taken ·0001 grm. as the unit of measurement. In the following experiments column A denotes the amount of the colouring salt present, B the depth of the disc, and C the amount of the colouring salt thence derived by calculation. Standard solution 35·2 of permanganate of potash in 500 cc. of water depth of disc, 8·3.

A	B	C
53	6·1	48
70	4·1	71

Experiments were made with still stronger solutions but the differences were considerable.

Standard solution 24 of permanganate of potash in 500 cc. of water, depth of disc, 8·3.

A	B	C
36	5·4	37
48	3·6	55
60	2·8	71

Still stronger solutions were used with this standard, but the discrepancies between the true and the calculated quantities were considerable. Standard solution 5·8 in 500 cc. of water, length of column, 8·3.

A	B	C
7·2	5·7	8·4

Some experiments were made with solutions of permanganate of potash, using a shorter column for the standard solution. Standard solution 2·4 in 214·6 cc., length of column 3·8.

A	B	C
3·6	2·2	4·1

An experiment was also tried with a still weaker solution. Standard solution 1·4 in 214·6 cc. of water, length of column 3·8.

A	B	C
2·4	2·6	2·1

Some experiments were also made by looking through the column of fluid at a white surface external to the cylinder. Standard solution 12 in 500 cc., length of column 8·4.

A	B	C
14·4	6·5	15·5
28·8	3·1	32·5
16·8	5·3	19

I prepared a fresh standard solution of the permanganate and compared the last solution with it. The new standard was similar to the last in composition.

A	B	C
16·8	5·7	17·6

Thus the result was somewhat more favourable with a newly-prepared solution. I used the permanganate on account of its great tinctorial power, but it is a salt of considerable instability, especially in contact with organic matter. The water used in the experiments was obtained by distilling Manchester town's water from a copper still lined with tin, and condensing in a tin condenser. Now such water would not be wholly free from oxidizable matter; hence in a little while there will be a depreciation in the tint of the permanganate solution. If the tint be strong the error arising from this cause might be neglected, but when the tint is feeble the error is appreciable. If the depreciation in tint in the two solutions compared were proportional to the depth of tint in each, this source of error might be neglected; but inasmuch as the same quantity of water has been used in making each solution, an equal amount of depreciation is introduced into each. Some of the foregoing experiments will be affected by this source of error. Small sources of error which may be neglected when we deal with ponderable quantities become of sufficient magnitude to vitiate results when we deal with traces.

For experiments with a yellow solution I took a solution of bichromate of potash. Standard solution 10 in 500 cc. of water, depth 8·3.

A	B	C
15	5·4	15·4
20	4·4	18·8
30	3·5	23·7
40	2·5	34·0
50	1·7	48·8
60	1·5	55·3

Still stronger solutions of the bichromate were compared with this standard, but the discrepancies were considerable, Some of the results also given in the last table are not very good; this more especially applies to the third and fourth results. On another occasion I repeated some of these experiments with the following results—standard solution same as last:

A	B	C
30	3	27·6
40	3·2	37·7
50	1·6	51·8
60	1·4	59·3

Another series of experiments was made with the bichromate. Standard solution 50 in 500 cc., length of column 8·8.

A	B	C
60	6·7	62
70	5·3	78
80	4·5	92
90	4	104
100	3·7	112
110	3·4	122
120	3·2	130
130	3	138
140	2·8	148
150	2·6	160

In some of these determinations I felt considerable uncertainty at what depth equality of colour was obtained, and some of the results were likely to vary on repetition, In the table I put 5·3 as the depth after some hesitation, and with the intention to repeat the experiment on some future occasion. On repetition I made the depth of the disc 5·8, which would indicate the amount of the salt used as 71. In some other instances I felt uncertainty when working with a solution of the bichromate in determining equality of tint, so that whether the disc were elevated above or depressed below the position finally chosen the difference in colour was insensible. Perhaps with bright yellow colours the eye requires more experience to detect differences of shade than with colours not so glaring. Moreover, when there is a considerable difference between the quantities of the colouring salt in the two cylinders there is another difficulty to deal with. The colours to be compared differ not only in intensity but also in kind. A very dilute solution of bichromate gives a pale yellow with a slightly greenish shade, which passes, as the amount of salt is increased, into full yellow, orange yellow,and finally orange. A similar observation holds with regard to several other colouring salts. Thus a very dilute solution of permanganate of potash is pink, passing, as the strength of the solution is increased, into crimson, and finally into violet. I also made some experiments with a blue solution. For this purpose I used an ammoniacal solution of sulphate of copper. The tinctorial power of this salt is not as great as I expected, and I made no experiments with a standard solution containing less than 150. On diluting 15 cc. of an ammoniacal solution with sufficient water to make 500 cc., I noticed after a little time that the solution was somewhat turbid. This was owing to the copper forming an insoluble compound, either hydrate or a subsalt; further addition of ammonia was necessary to clarify the solution. Standard solution 150 in 500, depth 8·3.

A	B	C
200	5·7	218
220	5·3	235
240	5·1	244
260	4·8	259
280	4·5	277
300	4·1	304
350	3·4	366
400	3·0	415
450	2·6	479

Some experiments were made with solutions of known strength, by sinking the disc to the depth assigned by theory, to see if the given tint was attained, instead of moving the disc about until the tint was estimated to be the same and then reading off the result. A solution of bichromate of potash containing 50 in 500 cc., and depth 8·3, was used as a standard. Solutions containing 60, 70, 80, 90, gave similar tints at the calculated depths. Solutions containing 150, and increasing quantities gave tints a little too deep at the calculated depths. Also a standard solution of permanganate of potash, containing 4·8 in 500 cc. of water, and depth 8·3, was taken. Solutions containing 6 and 7·2 gave a similar tint at the depths assigned by the calculation. Solutions containing 9, 9·6, and higher quantities, gave tints a little too deep at the calculated depths.

Some of the foregoing results are fair approximations, and probably better results might be obtained with a more perfect form of apparatus than I employed. The surfaces used were not perfectly flat. Moreover, there was in some of the experiments the indecision arising from the coexistence of several distinct tints on the surface of the disc due to the formation of caustic curves. This I afterwards remedied by working behind a screen of lawn. The effect of this is to render the illumination of the disc much more uniform. Instead of a screen we might also use glass cylinders ground internally. Another difficulty is the indecision respecting the depth at which equality of colour takes place. The degree of indecision will be different for different persons, and I suppose represents want of sensibility and want of skill in detecting shades of colour. So far as it arises from want of skill it is likely to be diminshed by more perfect experience with colours. It seems to me that it would be well to have some definite method for exercising the eye, or for testing one's skill in this respect. This might be done by having a series of cylinders filled with coloured solutions, the quantity of colouring matter increasing in arithmetical progression and endeavouring to arrange these solutions in their order, the arrangement being made according to the judgment of the tints. The various success attending such exercises would afford the experimenter some method for testing his skill from time to time; it would also show him under what conditions he could use neither this nor any other method of colorimetry; for when he has arrived at such a stage that he can perceive no difference of tint, notwithstanding an increase in the colouring matter, it seems as useless to try to do so as to use a balance of coarse sensibility to distinguish between two weights which are very nearly the same. By frequent exercise and concentrated attention I think the power of perception of difference of shade may be increased. If such a series of cylinders were taken containing, say quantities of bichromate of potash, increasing from 1 to 30, the first members of the series would be easily arranged towards the middle of the series, the number of successful discriminations would diminish and few would be able to detect differences in the higher members of the series; for an increment of the salt which produces a considerable difference in intensity when a small quantity of the salt is present produces a small difference when the quantity is large. This would follow from the approximate formula used, for $\frac{dt}{dQ} = -\frac{c}{Q^2}$ (t = depth, Q = quantity of colouring matter, c = constant. So that for a given small difference in the quantity the difference in the depth would be nearly proportional to the inverse square of the quantity. Solutions whose quantities of colouring matter are so similar as to be beyond discrimination by colorimetry may be again brought within its range by the following device: in each of the two cylinders destroy by some appropriate method an equal quantity of the colouring matter; then their differences will be increased relatively to the quantities of colouring matter remaining, and may be sufficiently sensible to be estimated. When I suggested this method of colorimetry I had not seen the article on Light by Sir John Herschel in the Encyclopædia Metropolitana. He there gives a probable formula for the intensity of light which has passed through an absorbing medium. His reasoning is as follows. Let the intensity of the incident light be unity, and its intensity on emerging from a unit thickness of the medium, k; then its intensity at incidence on a second layer will be k_1 and its intensity at emergence k^2; hence if t be the thickness of the medium the intensity will be finally k^t, and if the original intensity were a the final intensity would be ak^t. Now, light consists of several species; hence if the composition of the incident light be $a_{I} + a_{II} + a_{III} +$ &c. the composition of the emergent light will be $a_{I}k_{I}^{t} + a_{II}k_{II}^{t}$

$+a_{III}k^t_{III}+$&c. If we use the letter I to denote incident light, T to denote transmitted light, and A absorbed light, Herschel's formulæ may be written more briefly as follows:

$$\Sigma a = \mathrm{I}, \ \Sigma ak^t = \mathrm{T},$$

k will be a function of the quantity of colouring matter present in a unit thickness. If g be that quantity we may write $k=f(g)$, or if we expand $k=f(0)-f'(0)g+$ higher terms in g. Now, g is very small. Let then the terms higher than the second be neglected. The first term will be the light transmitted when no colouring matter is present. If the colouring matter were dissolved in an absolutely transparent medium, the first term would be unity. In practice water or some colourless medium is used; but the amount of light absorbed by a thin layer of pure water is very small, so that we shall make only a very small error in writing $f(0)=1$. Hence $k=1-f'(0)g$. Substituting this value in Herschel's formula and expanding, we have

$$\Sigma a - \Sigma af'(0)gt + \&c.$$

Now, as g is very small, we may neglect terms beyond the two first (unless t be very large). Hence

$$\Sigma af'(0)gt = \mathrm{I} - \mathrm{T} = \mathrm{A}.$$

If we assume the colour to be constant, A will be a constant term. Now, let Q denote the whole quantity of colouring matter in the cylinder, and L the whole length of the column; then $g=\frac{\mathrm{Q}}{\mathrm{L}}$ and the formula may be written

$$\Sigma af'(0)\frac{\mathrm{Q}.t}{\mathrm{L}} = \mathrm{A}, \text{ or} \mathrm{Q}t = \frac{\mathrm{A.L}}{\Sigma af'(0)} = \text{constant.}$$

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Annual Meeting, April 8th, 1878.

CHARLES BAILEY, Esq., in the Chair.

The Treasurer's Account and Report of the Council were read and passed.

The Election of Officers for the ensuing year then took place as follows:—

President.

C. BAILEY.

Vice-Presidents.

THOMAS ALCOCK, M.D.
H. A. HURST.
A. BROTHERS, F.R.A.S.

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Nix, E. W., M.A.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S.
Smart, Robert Bath, M.R.C.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist.,Owens College.
Wright, William Cort.
Waters, A. W., F.G.S.

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Stirrup, Mark, F.G.S.
Tatham, John F. W., M.D.
Waterhouse, J. Crewdson.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER,

Dr. IN ACCOUNT WITH H. A. HURST, TREASURER. **Cr.**

		£	s.	d.
1877				
Nov. 10.—	To H. Young, Jardine's Nat. Library	8	18	6
,,	,, J. Collins & Co., Note Paper	0	13	6
,,	,, J. Rowsell, Nat. Pd. Seaweeds and Flora Victoria	8	19	0
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	,, J. Van Voorst, Subscription to "Ibis" for 1878	1	1	0
,,	,, W. Wesley, Hewitsons's Butterflies.	9	0	0
,,	,, Subscription to Parent Society	2	2	0
,,	,, Postages	0	18	9
,,	,, Teas	3	3	4
,,	,, J. B. Dancer	0	3	6
,,	,, R. H. Porter, "Ibis," 1859-76	40	0	0
1878.				
April 8.—	Balance with Manchester and Salford Bank	108	14	10
		£209	11	1

		£	s.	d.
1877.				
May 1.—	By Balance at Banker's	84	16	8
,,	,, Subscriptions	22	10	0
,,	,, Interest allowed by Bank	1	4	5
,,	,, Subscriptions—J. Sidebotham, 10s., J. Labray, 10s.	1	0	0
1878				
April 6.—	,, Natural History Fund, on account	100	0	0
		£209	11	1

Audited and found correct,

(Signed) J. Cosmo Melvill,

A. Brothers.

(Signed) H. A. Hurst.

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PROCEEDINGS

OF THE

MANCHESTER

LITERARY AND PHILOSOPHICAL SOCIETY.

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NOTE.

The object which the Society have in view in publishing their Proceedings is to give an immediate and succinct account of the scientific and other business transacted at their meetings to the members and the general public. The various communications are supplied by the authors themselves, who are alone responsible for the facts and reasonings contained therein.

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PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY.

General Meeting, October 1st, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. James Ballantyne Hannay, F.R.S.E., Assistant Lecturer on Chemistry in the Owens College; and Mr. John Priestley, Demonstrator and Assistant Lecturer in Physiology and Histology in the Owens College, were elected Ordinary Members of the Society.

Ordinary Meeting, October 1st, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Among the Donations announced was a portrait of Dr. Percival, presented by Mr. Francis Nicholson, F.Z.S., Honorary Librarian of the Society.

On the motion of Mr. C. BAILEY, seconded by the Rev. WM. GASKELL, it was resolved That the cordial thanks of the Society be given to Mr. Nicholson for his valuable donation of a portrait of Dr. Percival, the first President of the Society.

Ordinary Meeting, October 15th, 1878.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. Binney, F.R.S., F.G.S., said that at a meeting of the Society held on the 14th day of November, 1876, he gave an account of a *Eucalyptus globulus* growing in his garden at Douglas in the Isle of Man. In that year (1876) it had grown 7ft. 8in. in height. In the following year 6ft. 8in., when it unfortunately lost its leading shoot by accident. Up to the 7th of this month it had grown 6ft. Thus in the three years it had reached 20ft. 4in. in height. The tree was planted in a sheltered situation close to the sea, and its foliage at the present time, notwithstanding the recent severe gales, presents a most luxuriant appearance and yields a pleasant odour. No manure has been supplied to the roots, which have grown in a soil formed of the *debris* of Manx schist.

"Relative Brightness of the Planets Venus and Mercury," by James Nasmyth, C.E., F.R.A.S., Corresponding Member of the Society.

On many occasions when observing Mercury and Venus separately in full daylight, I have always been impressed with the strikingly inferior brightness of Mercury as compared with Venus; and as such a condition is the very reverse of what might be expected by reason of Mercury being so much nearer to the sun than Venus, I awaited the rare event of a very close conjunction of these two planets that occurred on the 26th and 27th of September last. With the advantage of a perfectly clear sky I had the two planets before me for several hours, so to speak, side by side in the field of the telescope at the same time, thus affording me a most perfect opportunity for making a comparison of their relative brightness. It is difficult to convey in words an exact impression of the difference in the brightness of such objects, but I may attempt to do so by stating that Venus looked like clean silver while Mercury looked like lead or zinc. Were I to indicate my impressions by way of number I would say that Venus was fully twice as bright as Mercury. So remarkable an inferiority in the brightness of Mercury, notwithstanding his much greater nearness to the sun, appears to me to indicate the existence of some very special and peculiar condition of his surface in respect to his capability of reflecting light, a condition that may be due to the nature of his envelope, if such exist, or of that of his surface, by which the fervid light of the sun's rays falling on him are in a great measure quenched or absorbed so as to leave but a small residue to be reflected from his surface. If this be so, it appears to me to be reasonable to suppose that the absorption of so much light must result in a vast increase in the heat of the surface of Mercury beyond what would have been the case had Mercury possessed the same surface conditions as Venus. Whether in the progress of spectroscopic investigation we shall ever be enabled to detect some evidence of metallic or other vapours or gases clinging to or closely enveloping the surface of Mercury that might in some respect account for so remarkable an absorption of the sun's light, we must be content to await the acquirement of such evidence if it ever be forthcoming. It appears to me, however, to be well to raise such a question, so that our astronomical spectroscopists may be on the outlook for some evidence of the cause of so very remarkable a defective condition in the light-reflecting power of Mercury to which I have thus endeavoured to direct attention.

"On the Water of Thirlmere," by HARRY GRIMSHAW, F.C.S., and CLIFFORD GRIMSHAW.

The samples of water of which the following details are given were taken by us on August 23rd, 1878, from what may be termed the "upper and lower lakes," meaning, above and below the narrow waist of the lake, where crossed by the small bridge. The first taken was the "lower" sample, which was obtained on the west side of the lake about twenty yards below the bridge. The "upper" sample was taken about three quarters of a mile below the head of the lake on the east side. The results of the two samples so far as position affects them should therefore be sufficient to show any possible divergency which could occur in the quality of the water on that account. These two positions are shown upon the sketch map accompanying the paper.

The two samples were analysed separately except as regards the amounts of ammonia (determined August 28th), for which equal parts of each sample were taken, as there was not a sufficient quantity for separate analyses.

The following are the numbers obtained:

Free ammonia—half a litre of water distilled took 1·5 cc. standard NH_4Cl.

Albumenoid ammonia—half a litre of water distilled took 3·0 cc. standard NH_4Cl.

Total hardness—70 cc. water took 1 cc. standard soap solution.

Permanent hardness—70 cc. water boiled 1 hour and made up took 1 cc. soap.—(The ammonia analysis common to both samples, hardness same in each).

Solid matter—100 cc. lower sample gave 0·0045 grams residue, which blackened on ignition and lost 0·0025 gram.

Solid matter—70 cc. upper sample gave 0·0022 grams residue, which blackened a little on heating, not so much as lower sample, and lost on ignition 0·0007 gram.

Chlorine—25 cc. lower sample took (1st) 0·25 cc. (2nd) 0.25 cc. standard $AgNO_3$; 25 cc. upper sample took (1st) 0·15 cc. (2nd) 0·15 cc. standard AgNO .

The foregoing figures give the following tabulated results :—

	Grains per Gallon.		
	Lr. Sample.	Up. Sample.	Dr. Roscoe's.
Total solid matter	3·15	2 20	1·45
Mineral matter	1·40	1·50	—
Loss on ignition	1·75	0·70	—
Total hardness	1·00	1·00	0·50
Permanent hardness	1·00	1·00	0·50
Chlorine	0·70	0·42	0·42
Nitrogen as nitrates and nitrites	—	—	0·0247
Free ammonia	0·0021		0·00
Albumenoid ammonia	0·0042		0·0049

The "ammonias" expressed in parts per million are 0·03 and 0·06 respectively; the chlorine equals 0·7 and 1·16 grains per gallon of salt.

The water was very clear and sparkling, practically free from sediment, and its reaction perfectly neutral to litmus. There was no reaction for heavy metals, copper, lead, iron, &c., although we observed that there ran in close to the top of the lake a small stream of a very turbid milky appearance, upon which is situated on the side of the hill towards Helvellyn, a lead mine; which stream could be traced by its colour for at least 20 yards into the clear water of the mere. These mining operations we understand will of course be completely done away with on proceeding with the water scheme.

We may offer the following brief remarks upon the above analysis of the water of Thirlmere. In the first place it is evident that the water is one of the very purest description found in nature, not being surpassed by that of any locality in Great Britain of which analyses have been published.

As regards its use for manufacturing purposes it practically could not be improved. For drinking purposes also it is perfectly free from anything which could be considered objectionable, of either an organic or mineral nature, though we are not prepared to assert that a water considered "solely" as to its suitability for this purpose attains any particular advantage from having its amount of lime salts lower than some 10 or so grains per gallon, though doubtless it is better that this should be very small, than that it should be excessive. To give a fuller indication of the quality of the water of Thirlmere, we have taken the liberty of appending to our results, those obtained by Dr. Roscoe from a sample of the same taken in February of this year. Now it will be seen that there is a certain difference between the results of these samples, more especially with regard to our "lower" one. On examination, however, the difference will be seen to be one of "degree" only, and not of "kind," and entirely confined to the mineral, and in this case the least important items. It is explained by the fact that the rain-fall some little while before the taking of the respective samples had been very different, the longest period of dryness perhaps which we have had this year having occurred just before the taking of those of the present paper. We should therefore expect that the solid matters would be higher, and find them to be so by about one and a half grains per gallon, the total in either case being insignificant. It would perhaps not be wide of the mark if we said that the analyses of Dr. Roscoe and ourselves show the water at its best and its worst as regards ordinary variation. This variation it will be seen is not greater than may occur at the same time in different parts of the lake, as shown by our upper and lower samples, these analyses also indicating a slight gradual increase of dissolved material as we approach the foot of the lake, where the solvent action of the water upon its bed will have accumulated and the fresh water received from the tributaries is lowest in amount. This gradation of quality will, of course, only be true of the lake under its present natural conditions.

The variation of the organic constituents of the water at the different dates is not so marked. From the behaviour of the solid matter during ignition we should say that the carbonaceous matter, of which there is a perceptible amount, is almost entirely of a vegetable nature.

Ordinary Meeting, October 29th, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Note on certain Thionates," by E. J. BEVAN, Student in the Owens College. Communicated by Professor H. E. ROSCOE, F.R.S., &c.

Thallium Triothionate. When thallium carbonate is treated with an equivalent quantity of trithionic acid, prepared by precipitating potassium trithionate with tartaric acid, and the aqueous solution so obtained evaporated over strong sulphuric acid, thallium trithionate separates out in long colourless needle-shaped crystals isomorphous with those of the corresponding potassium salt. Thallium trithionate decomposes slowly at the ordinary temperature, quickly on heating, and is with difficulty prepared free from sulphate. Analysis showed it to contain 68% of thallium, whereas the formula $K_2S_3O_6$ requires 67·95%.

Hypovanadic Dithionate.—If an aqueous solution of barium dithionate be precipitated with the requisite quantity of hypovanadic sulphate, and the blue filtrate from the barium sulphate concentrated in a vacuum over strong sulphuric acid, crystals of hypovanadic dithionate separate out. If concentration be continued after the first appearance of these crystals, decomposition takes place, sulphur dioxide is set free, and hypovanadic sulphate remains. This easy decomposition prevents hypovanadic dithionate being prepared in the pure state.

Aniline Dithionate—This salt is obtained in beautiful long needles, by the double decomposition of barium dithionate with aniline sulphate. Aniline dithionate is comparatively stable, its aqueous solution may be boiled without decomposition, when the dry salt is cautiously heated in a vacuous tube it sublimes, a portion, however, undergoing decomposition. It is freely soluble in water and alcohol; 100 c.c. of water at 16° dissolve 7·89 grms. of the salt. Its alcoholic solution, treated with sodium amalgam, yields aniline sulphite. Analysis showed this body to contain 18·60% of sulphur, whilst $(C_6H_3NH_2)_2$ $H_2S_2O_1$ requires 18·40% S.

"The Use of the Opercula in the Determination of the Cheilostomatous Bryozoa," by **ARTHUR WM. WATERS**, F.G.S.

In the determination of the Bryozoa various authors have attached very different importance to the characters used, but the form of the aperture has been recognised by all to be of use. D'Orbigny rightly used this very largely, and Hassall pointed out that it was much less varying than many points. Busk of course mentions the form in most cases, but Professor F. A. Smitt, the first authority on the Bryozoa, has used it much more largely, and has based his classification of the Lepralia and allied genera principally upon the form of the aperture. Dr. Hincks also has recently proposed a new classification in many points similar to Smitt's, in which the form of the aperture is largely made the basis.

It seemed to me that as the shape of the oral-aperture can only indicate the form of the operculum, which closes it, the opercula themselves might furnish more reliable information, and I therefore prepared a series from the material I had available, and I find it may be even more useful than I at all expected. Besides indicating the form of the aperture, there are also many other characters shown, so that in the first 35 examined and figured all have an easily distinguishable operculum.

The operculum is closed by means of two muscles, which in some cases, as Myriozoum truncatum, fig. 15; Lepralia pertusa var., fig. 4; Lepralia arrogata, fig. 6, &c., &c., are attached at the side, in some quite from the edge, in others, as Cellepora, fig. 8, from a muscular boss on the rim; but in most of the Celleporidæ, and some Lepralia, &c., the muscular attachment is on the interior surface as in Lepralia Cecilii, fig. 1; Cellepora, fig. 5, &c.

In many it would seem that the opercula move upon a sort of hinge, as in Myriozoum truncatum, fig. 15; Lepralia arrogata, fig. 6, and a large number figured. In others it seems that a membrane attached at the proximal end retains it in its place. The opercula of some are curved into a saddle-shaped form, as Retepora Couchii, fig. 22, Eschara foliacea in stadium Eschara and Hemeschara, fig. 24, 25, and these have a concave proximal edge.

In some the operculum consists of two separable layers, and this apparently causes the peculiar light portion of a part in Lepralia cucullata. In Tubucellaria cereoides a light oval patch is seen in the centre, and in section fig. 36 this is shown to be caused by the chitinous operculum being double elsewhere; in fact it is like a meniscus with a concave stop in front. I have every reason to think there is no membrane enclosing this oval space, but cannot speak with certainty, as I have confined my attention to those points I deemed useful in determination, though a complete study of the structure of the opercula, generally, would no doubt repay the trouble. Tubucellaria hirsuta is not double in the same way, and has the long muscular bosses further from the edge.

In a large number of the Bryozoa the old and young zoœcia are known to vary so much as not to be recognisable as the same species if discovered separately, but the opercula of both are the same; also the operculated aperture is in many surrounded by an elevated peristome, in other words, is deep down in the throat, so that the shape cannot be seen on account of the projecting lips. In such a case the examination of the operculum will reveal the true form.

The determination of the Celleporidæ has seemed to be an almost hopeless task, as the form of the colony varies greatly, and the avicularia and other characters are difficult to use, but I believe the opercula may assist very much to bring this family out of its present confusion. In Cellepora the outside cells are decumbent, while the central ones are erect, so that often no similarity is apparent, but so far as my examination goes the opercula of both stages are the same.

The size seems to vary but little, and in a slide of 6 or 7 in many cases all would exactly correspond with the drawing of the one figured, and in few cases was the variation over a tenth of the size. I was also much surprised to find how closely the measurements of these Naples specimens corresponded with the sizes of the aperture as given by Smitt in his description of the Northern forms.

Although only groups allied to Lepralia have now been brought under examination, it is by no means in these families only that the shape of the oral aperture is of importance, as in nearly all genera it is specifically characteristic as may be seen in Membranipora, Diachoris, Bugula, Flustra, &c., and a little more attention to this would have made the British Museum Catalogue of much more value, and prevented uniting under one name as Lepralia spinifera species with different shaped apertures, which it has since been necessary to separate. There are also other points which may be of use, as for instance I find in one Eschara from Australia that the operculum has 7 teeth in front, the central one the largest, and in Cellepora sardonica, fig. 27, it is strange to find lines on the edge corresponding with the minute teeth in the aperture.

I should propose to divide the opercula into (*a*) those with a straight proximal edge 27, 28, 29, 30, 31, 34; (*b*) those with a straight edge and a sinal projection 1, 2, 3, 4; (*c*) those with a subtriangular proximal end 5, 6, 7, 8, 15; (*d*) those with a rounded proximal end 9, 10, 11, 12, 16; (*e*) saddle shaped, mostly with a concave end 22, 23, 24, 25, 26; (*f*) suboblong 17, 18, 19, 20.

That the opercula may be of the greatest use in specific determination there is now no doubt, and thus intimate

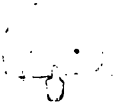
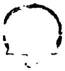


Fig 1.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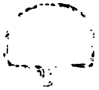
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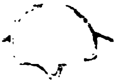
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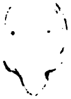
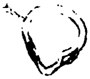


Fig 5.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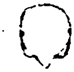
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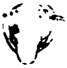
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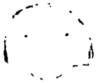
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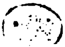
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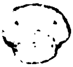
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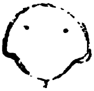
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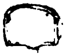
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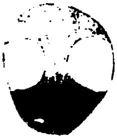
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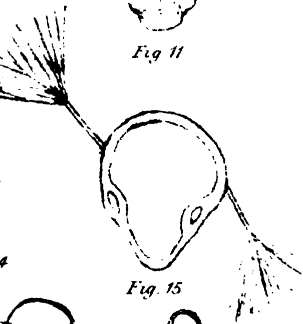
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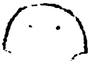
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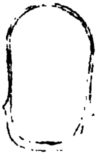
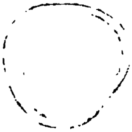


Fig 17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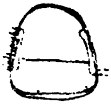
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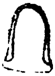
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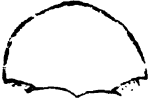
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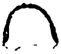
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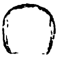
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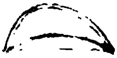
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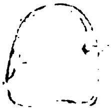
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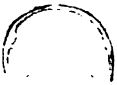
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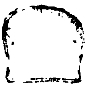


Fig 27.

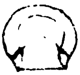


Fig 28.

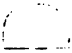


Fig. 29.

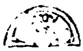


Fig. 30.

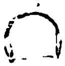


Fig 31

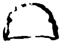


Fig. 32.

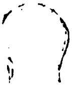


Fig 33

Fig. 34.

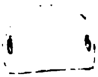


Fig 35

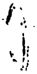


Fig 36

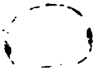


Fig. 37.

relationship will be indicated, but how far general classification may be assisted must remain open until larger series have been examined.

EXPLANATION OF PLATE.

OPERCULA OF

1 Lepralia Cecilii, Aud.
2 ——— vulgaris, Moll.
3 ——— pertusa, var.
4 ——— ———, var.
5 Cellepora.
6 Lepralia arrogata, Waters.
7 ——— linearis, Hass. var typica Smitt.
8 Cellepora.
9 Lepralia errata.
10 Cellepora Hassallii, John.
11 ——— verruculata, Smitt.
12 Cellepora.
13 Lepralia auriculata, Hass.
14 ——— cucullata, Busk.
15 Myriozoum truncatum, Pall.
16 Lepralia.
17 Lepralia Pallasiana, Moll. var projecta, W.
18 ——— coccinea, Abild.
19 ——— Pallasiana, Moll.
20 ——— lata, Busk. This is larger than most opercula of lata.
21 Eschara contorta, Busk.
22 Retepora Couchii, Hincks.
23 ——— cellulosa, Jam.
24 Lepralia foliacea, stadium Hemeschara.
25 Eschara foliacea, Ell & Sol.
26 Lepralia reticulata, Macg. var ophidiana, W.
27 Cellepora sardonica, Waters.
28 Eschara verrucosa, Peach.
29 Lepralia ciliata, Pall.
30 ——— Malusii, Aud.
31 ——— bimucronata, Moll.
32 ——— Brongniartii, Aud.
33 ——— recticulata, Macg. var inequalis, Waters.
34 ——— Gattyae, Lands.
35 Tubucellaria hirsuta, Busk.
36 ——— cereoides, Ell & Sol. (in section).
37 Ditto do.

All magnified 85 times.

"On some Improved Methods of Producing and Regulating Electric Light," by Henry Wilde, Esq.

Among the manifold functions which the elementary substance carbon performs in organic nature, not the least important is that by which it becomes the great source of artificial illumination, whether derived from oils, coal gas, or from coke rendered incandescent by the action of powerful electric currents. Since the time when Davy first produced the voltaic arc, between two points of wood charcoal, through which was transmitted the current from the great battery of 2000 plates belonging to the Royal Institution, many experiments have been made to determine the best kinds of carbon for developing the electric light. The carbon which, until recently, was most commonly employed for this purpose, is obtained from the sides of gas retorts, where it accumulates in the form of coke during the destructive distillation of coal. The shells of coke from the retort are sawn up into pencils from one quarter, to half an inch square, and from six to nine inches in length. Although very good results are obtained from carbon of this kind, it is a difficult material to work on account of its hardness, and it sometimes contains impurities which interfere with its conductivity. It is also liable to fracture when suddenly heated by the transmission of powerful electric currents. These defects have led to the introduction in electric lighting of artificial carbon, composed of powdered coke and lamp black, formed into a paste with molasses and gum. This material is pressed into cylindrical forms, and subjected for a given time to a high temperature in a special furnace. The manufacture of these carbon pencils has attained great perfection in the hands of Carré of Paris, and they can be made into perfectly straight and cylindrical forms of from two to sixteen millimètres in diameter, and half a metre in length

When the electric light is to be used for illumination, it is necessary that it should be as continuous as other modes of lighting. For this purpose not only should the current be regular in its action, but the distance between the carbon points must not alter, which necessitates the use of some arrangement for bringing them nearer together in proportion as they are consumed. Much ingenuity has been displayed by electricians in solving this problem, and the automatic contrivances invented by Staite, Duboscq, Foucault, Serrin, and others, leave little to be desired in regard to the steadiness of the light, when the regulators are in good order, and in the hands of intelligent operators. All automatic instruments, however, from the delicacy of their mechanism, are liable to derangement, and their action is not easily understood by persons not having a special knowledge of their construction. To obviate the objection to the use of such instruments by unskilled attendants, I devised, a few years since, a regulator for use on H.M.'s ships of war, to be actuated by hand. In this arrangement the carbons are made to approach and separate from each other by means of a right and left handed screw connected with the carbon holders. Each of the screws, with its carbon holder, can be actuated independently of the other, for the purpose of adjusting the points of the carbons to the proper focus of the optical apparatus used in connection with it. The regulator, with its carbon points, is placed in the focus of a dioptric lens, which parallelises the divergent rays of light into a single beam of great intensity. The lens with the regulator is pivoted horizontally and vertically on the top of a short iron column, fixed on a raised platform above the deck; and the beam of light may be projected upon any distant object within its range. This special application of the electric light, however, as will be seen, requires the frequent adjustment of the carbons by the operator; but as he is always required to be in attendance to manipulate the projector, no inconvenience is experienced through the absence of the automatic arrangement.

This method of regulating the electric light has now been in use in the Royal Navy for more than three years, and has proved very satisfactory.

Simultaneously with the progress of improvements in the mechanism for regulating the electric light, experiments have been made with the object of dispensing with the regulator altogether. The most recent, as well as the most successful, of these attempts has been made by M. Jablochkoff, a Russian inventor. In the specification of his letters patent of 1877, he proposes to place the carbons side by side (as had been previously proposed by Werderman in 1874), and to separate them by an insulating substance to be consumed along with the carbon. The inventor states that the insulating substance for separating the carbons may be kaolin, glass of various kinds, alkaline earths, and silicates, which he prefers to apply in the form of powder rammed into an asbestos cartridge case containing the carbons. A powder which the inventor found serviceable, consists of one part lime, four parts sand, and two parts talc. These materials are rammed into the cartridge case surrounding and separating two parallel sticks of carbon placed in the case, at a little distance apart. One of the carbons is made thicker than the other to allow for its more rapid waste. The lower ends of the carbons are inserted into pieces of copper tube or other good conductor, separated from one another by asbestos, and the ends of the tubes are pinched between two limbs of a screw vice, connected respectively to the conducting wires. This combination of carbons and insulating materials the inventor terms an electric candle, which, when mounted on a stand or candlestick, has the appearance of the Roman candle of pyrotechnists. The inventor further states that the heat produced by the electricity fuses the material between the carbons and dissipates it; and the freedom of the passage afforded by the fused material to the electric current permits the subdivision of the light by placing several lamps in the course of one electric circuit. It is also stated that the construction of the candle may be varied; and, among the forms described, is one in which the carbons, instead of being contained in a cartridge case, are separated by a partition of kaolin or other similar insulating material.

I have thought it well to describe, as nearly as possible in the words of the inventor, the electric candle, which is now the subject of so much attention in its application to electric lighting; so that its relation to what follows may be more clearly perceived. A remarkable peculiarity of the direct current in electric lighting is that of its consuming the positive carbon at twice the rate of the negative one, and while the negative carbon is a pointed cone, like that of a pencil, the positive pole takes the form of a hollow cavity or crater.

M. Jablochkoff's early experiments seem to have been made with the direct current, and hence his carbons are described as being of unequal thickness in order that the positive and negative carbons of the candle might be evenly consumed. When the alternating current is used for producing electric light both carbons are of the same thickness, and are consumed at an equal rate, and both points terminate in regular cones. This property of the alternating current, besides other advantages, always maintains the luminous point in the focus of any optical apparatus used in connection with it, that is, when the carbons are placed end to end, as I had occasion to point out in a former paper read before the Society in 1873, on an Electro Magnetic Induction Machine for producing alternating currents.

M. Jablochkoff, in the course of his experiments, would appear to have met with some difficulties in adapting the direct or continuous current to a system of lighting with his electric candles, and now uses the alternating current for this purpose. The candle has also been simplified by substituting a slip of plaster of Paris for the cartridge and partition of kaolin formerly employed.

To produce the alternating currents, however, to supply a number of lights, it was found necessary to employ powerful electro magnetic induction machines, excited by the currents from other smaller machines, according to the principles laid down in my paper read before the Royal Society, and published in the Philosophical Transactions of 1867. From 16 to 20 lights are produced from one of these electro magnetic machines, each light absorbing about one horse power.

The system of electric lighting above described would now seem to be definitely established in some places as a substitute for gas, and visitors to the French capital during the present summer, will have been struck with the fine effects produced in the avenues and squares where the light is displayed.

My connection with the history of this system of lighting, placed me in a position to make some experiments with the Jablochkoff candle, and led to the discovery of the following facts. One of the conditions necessary for producing a constant light from the candle, in its most recent form, was that the quantity and intensity of the alternating current should be such, that the carbons consume at a rate of from four to five inches per hour. If the electric current were too powerful, the carbons became unduly heated, and presented additional resistance to the passage of the current; the points at the same time lost their regular conical form. If, on the other hand, the current were too weak, the electric arc played about the points of the carbons in an irregular manner, and the light was easily extinguished by currents of air.

In the course of these experiments I was struck with the apparently insignificant part which the insulating material played in the maintenance of the light between the carbon points; and it occurred to me to try the effect of covering each of the carbons with a thin coating of hydrate of lime, and mounting them parallel to each other in separate holders, and without any insulating material between them. The use of the lime covering was intended to prevent the light from travelling down the contiguous sides of the carbons. On completing the electric circuit the light was maintained between the two points, and the carbons were consumed in the same regular manner as when the insulating material had been placed between them.

Two plain cylindrical rods of carbon three sixteenths of an inch in diameter, and eight inches long, were now fixed in the holders parallel to each other, and one eighth of an inch apart. The strength of the alternating current was such that it would fuse an iron wire 0·025 of an inch in diameter and eight feet in length. On establishing the electric current through the points of the carbons by means of a conducting paste composed of carbon and gum, the light was produced, and the carbons burnt steadily downwards as before.

Four pairs of naked carbons mounted in this manner were next placed in series in the circuit of a four-light machine, and the light was produced from these carbons simultaneously, as when the insulating material was used between them. The light from the naked carbons was also more regular than that from the insulated ones, as the plaster of Paris insulation did not always consume at the same rate as the carbons, and thereby obstructed the passage of the current. This was evident from the rosy tinge of the light produced by the volatilisation of the calcium simultaneously with the diminution of the brilliancy of the light from the carbons.

The only function, therefore, which the insulating material performs in the electric candle, as shown by these experiments, is that it conceals the singular and beautiful property of the alternating current to which I have directed attention.

As I have already said, the strength of the alternating current must bear a proper proportion to the diameter of the carbons used, and when a number of such lights are required to be produced in the same circuit, the quantity and property of the current will remain constant, while the tension will require to be increased with the number of lights.

This simple method of burning the carbons will, I believe, greatly further the development of the electric light, as the carbons can be used of much smaller diameter than has hitherto been possible. They may also be of any desired length, for as they are consumed they may be pushed up through the holders without interrupting the light. One of these developments will be a better method of lighting coal and other mines. In this application the alternating currents or waves from a powerful electro-magnetic induction machine may be used for generating, simultaneously, alternating secondary currents or waves in a number of small induction coils, placed in various parts of the mine. The light may be produced in the secondary circuits from pairs of small carbons enclosed in a glass vessel having a small aperture to permit the expansion of the heated air within. Diaphragms of wire gauze may be placed over the aperture to prevent the access of explosive gas. By generating secondary currents or waves without interrupting the continuity of the primary circuit, the contact breaker is dispensed with, and the subdivision of the light may be carried to a very great extent.

Ordinary Meeting, November 12th, 1878.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

E. W. Binney, V.P., F.R.S., said that on the 22nd January last he communicated to the Society a Notice of a Fossil Plant found at Laxey, in the Isle of Man, which was published immediately in the Proceedings. Now in *Nature* of the 31st of October appears a letter from Dr. Dawson, F.R.S., dated McGill College, Montreal, October 5, 1878, as follows: "A Fossil Plant—Misquotation. In an article on a fossil plant from the Isle of Man in *Nature*, Vol. XVIII. p. 555 (September, 1878), the following sentence is attributed apparently on the authority of Mr. Leo Lesquereux to my Report on the Devonian and upper Silurian plants of Canada 'that these fragments are probably originating in the upper Silurian of Gaspé; that as they are found in the lower part of the limestone which underlies the Devonian Gaspé sandstone and become more abundant in the upper beds, this suffices to indicate the existence of the neighbouring land probably composed of Silurian rocks and supporting vegetation.' On referring to the report in question I find that the original of this strange statement stands as follows: 'These remains of *Psilophyton* occur in the lower part of the limestone but are more abundant in the upper beds, and they suffice to indicate the existence of neighbouring land probably composed of lower Silurian rocks and supporting vegetation.' I have no doubt that Mr. Lesquereux quoted from memory, and probably supposed that he was expressing my meaning, but an English writer should have referred to the original. I may add that the specimen referred to in Mr. Binney's article does not exhibit the characters of my genus *Psilophyton,* which does not contain 'fucoids' but land plants of the rank of acrogens and of which not merely the external forms but also the internal structures are described and figured in the report referred to. The plant in question much more closely resembles *Buthotrephis Harknessii* of Nicholson from the Skiddaw slates."

With all due deference to Dr. Dawson I fail to see why I should send to London in order to consult his original report. Professor Lesquereux had sent me a copy of his (the Professor's) paper on *Psilophytum cornutum* published in the Transactions of the American Philosophical Society. This I received early in December, and immediately noticed the great resemblance, if not identity, of his *P. cornutum* with my Manx specimen. In noticing the latter I stated that I should give his description at length, and I accordingly gave it in inverted commas, word for word, including the part Dr. Dawson complains of. Surely he cannot seriously charge me with misquotation under such circumstances! However, the Professor's offence, if any, was published in the Transactions of the American Philosopical Society for November last year, I believe, and it appears the Doctor does not set the public and himself right by communicating with that society, but, without a moment's delay, in October of this year, sends over to *Nature* his so-called misquotation and its correction, adding that I "as an English writer should have referred to the original." Now as no direct reference was made by me to the Doctor or any specimen of his *Psilophyton,* none of which resemble the fossil plant described by me, I think he has no right to compel me to read over all his learned and voluminous works however much he might think that I should be benefited by so doing.

As to the Doctor's opinion that my Manx plant resembles the *Buthotrephis Harknessii* of Nicholson, all I can say is that I have carefully compared the two figures of the specimens—Dr. Nicholson having kindly sent me a copy of his paper published in the *Geological Magazine* for November, 1869—and I see no reason to change my views. My Manx plant is without question the most like Professor Lesquereux's figured specimen of *Psilophytum cornutum,* whatever that plant may prove to be, of all the fossil plants that have ever come under my notice. Any one may judge for himself by comparing the published figures of the specimens.

General Meeting, November 26th, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. Francis Jones, of the Manchester Grammar School; Mr. Sidney Trice Martin, No. 1, George Street, Manchester; Mr. Peter Phillips Bedson, D.Sc., Owens College; and Mr. Joseph Davies, Engineer, Manchester, were elected Ordinary Members of the Society.

Ordinary Meeting, November 26th, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

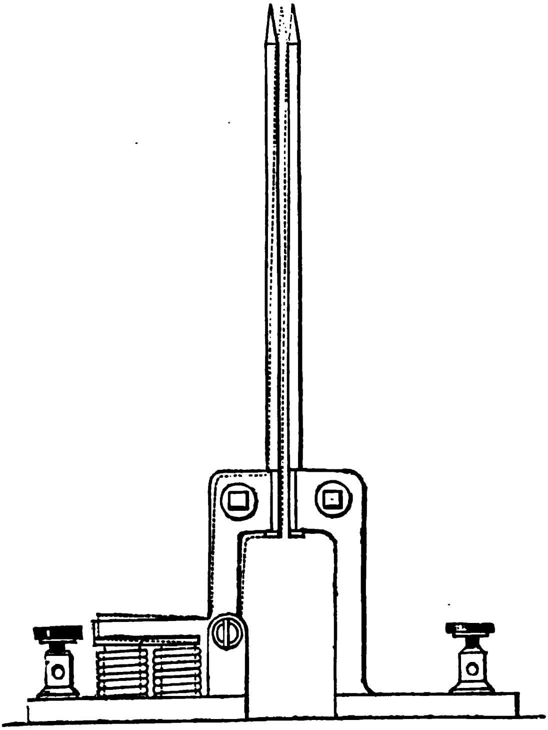
"On some Improved Methods of Producing and Regulating Electric Light," Part II., by HENRY WILDE, Esq.

In a former communication to the Society, I directed attention to the fact, that when the electric light is produced from the ends of two carbon pencils placed parallel to each other, if the strength of the electric current, the thickness of the carbons, and the distance between them are rightly proportioned, the carbons will burn steadily downwards until they are wholly consumed, without any insulating material between them. To initiate the light by this method, it is necessary to complete the electric circuit between the carbons by means of some conducting substance, which volatilises on the passage of the current, and establishes the electric arc between the points.

When a number of such lights are produced simultaneously from the same source of electricity, any interruption in the continuity of the current extinguishes all the lights in the same circuit, and each pair of carbons requires to be reprimed before the lights can again be established. This defect, as will be obvious, would cause great inconvenience when the lights are not easily accessible, or are at considerable distances apart.

In the course of my experiments it was observed that when the electric circuit was completed at the bottom of a pair of carbons close to the holders, the arc immediately ascended to the points, where it remained so long as the current was transmitted. My first impression of this peculiar action of the arc was, that it was due to the ascending current of hot air by which it was surrounded. This however was found not to be the cause, as the arc travelled towards the points in whatever position the carbons were placed, whether horizontally or vertically in an inverted position. Moreover, when a pair of carbons were held in the middle by the holders, the arc travelled upwards or downwards towards the points, according as the circuit was established above or below the holders. The action was in fact recognised to be the same as that which determines the propagation of an electric current through two rectilinear and parallel conductors submerged in contact with the terrestrial bed, which was described by me in the *Philosophical Magazine*, August, 1868.

In all the arrangements in general use for regulating the electric light, the carbon pencils are placed in the same straight line, and end to end. When the light is required, the ends are brought into momentary contact, and are then separated a short distance to enable the arc to form between them. The peculiar behaviour of the electric arc when the carbons are placed parallel to each other, suggested to me the means of lighting the carbons automatically, notwithstanding the fact that they could only be made to approach each other by a motion laterally, and to come into contact at their adjacent sides. To accomplish this object, one of the carbon holders is articulated or hinged to a small base plate of cast iron, which is so constructed as to become an electro-magnet when coiled with a few turns of insulated wire. The carbon holder is made in the form of a rightangled lever, to the short horizontal limb of which is fixed an armature placed over the poles of the electro-magnet. When the movable and fixed carbon holders are brought into juxtaposition, and the carbons inserted in them, the upper parts of the two carbons are always in contact when no current is transmitted through them, as shown by the dotted lines in the engraving.



The contact between the carbons is maintained by means of an antagonistic spring inserted in a recess in one of the poles of the electro-magnet, and reacting on the under side of the armature. One extremity of the coil of the electro-magnet is in metallic connection with the base of the carbon holder, while the other extremity of the coil is in connection with the terminal screw at the base of the instrument, from which it is insulated. The coils of the electro-magnet are thus placed in the same circuit as the carbon pencils.

When the alternating current from an electro-magnetic induction machine is transmitted through the carbons, the electro-magnet attracts the armature and separates the upper ends of the carbons, which brings them into their normal position, and the light is immediately produced. When the circuit is interrupted, the armature is released: the upper ends of the carbons come into contact, and the light is produced as before. When several pairs of carbons are placed in the same circuit, they are, by this arrangement, lighted simultaneously.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 7th, 1878.

Charles Bailey, Esq., President of the Section, in the Chair.

Professor W. C. Williamson, F.R.S., made remarks on a series of Lepidodendroid Macrospores from the Halifax Coal Measures, as well as a series of reproductive cryptogamic conceptacles from the same measures, and also exhibited some of these under the new Oil Immersion Lens of Zeiss of Yena.

Mr. Plant exhibited a so-called Tree Onion from Canada.

November 4th, 1878.

CHARLES BAILEY, Esq., President of the Section, in the Chair.

Mr. PLANT, F.G.S., exhibited as additions to the Fauna of Cymmeran Bay, Anglesea, fine specimens of the Portuguese Man of War (Physalia pelagica), which came in with a S.W. gale, accompanied by many specimens of the Vellella.

Mr. J. COSMO MELVILL, F.L.S., read an account of three visits paid by him to the Breidden Hills, Montgomeryshire, North Wales, in May, June, and July, 1877.

The Breidden hills form an isolated range in the extreme north-east corner of the county, where it joins Salop, about ten or twelve miles due west of Shrewsbury, and three miles distant from Welshpool, from which town the best view of them is obtained, and where they are the most conspicuous objects to the north-east.

The rocks are Lower Silurian, igneous and crystalline. Moel-y-Golfa, the southernmost peak, attaining an altitude of nearly 1500 feet, is formed of felspathic Trap and Ash; while Craig Breidden, the northernmost summit, and the intermediate hill, are both of Igneous Greenstone.

The county of Montgomery being one of the nine mentioned by Mr. Hewett C. Watson in his latest work—"Topographical Botany"—as being completely unworked, so far as Botanical data are concerned, renders any list of the plants interesting. Among a total of over 300 observed, the following catalogue represents the rarer species only:

Ranunculus parviflorus (L.)
Meconopsis Cambrica (Vig.)
Corydalis claviculata (D. L.)
Fumaria Boræi (Sm.)

Hesperis matronalis (L.)

Cheiranthus cheiri (L.)

Arabis—an hirsutæ varietas?—a form approaching A. arcuata of Grenin and Godron, and A. alpestris (Gor.), both of the flora of France—found sparingly by the waterfall, Craig Breidden.

Teesdalia nudicaulis (Sm.)

Lychnis Viscaria (L.)

Mœnchia erecta (Sm.)

Cerastium semidecandrum (L.)

Stellaria media (L.) var. neglecta.

Hypericum Androsæmum (L.)

Geranium sanguineum (L.)

Geranium lucidum (L.)

Trifolium filiforme (L.)

Spiræa salicifolia (L.)

Potentilla rupestris (L.) On damp ledges in almost inaccessible spots near the waterfall, Craig Breidden. The only British station.

Sedum Telephium (L.)

Sedum anglicum (Hudson).

Sedum Forsterianum (Sm.)

Cotyledon umbilicus (L.)

Saxifraga granulata (L.)

Saxifraga hypnoïdes (L.)

Helosciadium inundatum (Kosh.)

Heracleum sphondylium (L.) B. angustifolium.

Centranthus ruber (D. C.)

Doronicum Pardalianches (L.)

Centaurea montana (L.), naturalised near Welshpool.

Erigeron acris (L.)

Hieracium Pilosella (L.) B. pilosissimum, very hairy form without stolons, confined to Craig Breidden and the Channel Islands.

Hieracium lasiophyllum (Koch.)

Hieracium pallidum (Sm.) and its form *maculatum*.

Veronica hybrida (L.)

Veronica scutellata (L.)
Veronica montana (L.)
Ajuga reptans (L.) var. alba.
Euphorbia Lathyris (L.) nr. Criggion.
Ornithogalum umbellatum (L.)
Likewise the Laburnum—Cytisus Laburnum (L.) occurred in a quasi-naturalized state on Moel-y-Golfa.

Among *Ferns* and Fern allies the only ones observed were:

Pteris aquilina (L.)
Asplenium Ruta muraria (L.)
Asplenium Trichomanes (L.)
Asplenium adiantum nigrum (L.)
Athyrium Filix. fœmina (Berch).
Lastrea Filix. Mas. (Aich).
Polypodium vulgare (L.)
Polypodium Dryopteris (L.)
Equisetum arvense (L.)
Pilularia globulifera (L.)

Mr. M. M. **Hartog** (who was present as a visitor) said that when examining Desmids lately taken from a jar containing Chara, he had discovered a decided nucleus in the species known as Closterium aggregatum.

Ordinary Meeting, December 10th, 1878.

J. P. Joule, D.C.L., LL.D., F.R.S., President, in the Chair.

Among the Donations announced was a portrait of the late Professor Eaton Hodgkinson, presented by Sir **Thomas Fairbairn**, Bart.

On the motion of Mr. **Binney**, seconded by Dr. **Schunck**, it was resolved unanimously: That the thanks of the Society be given to Sir Thomas Fairbairn, Bart., for his valuable Donation of a portrait of the late Professor Eaton Hodgkinson.

"On the Combinations of Aurin with Mineral Acids," by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

In our last communication[*] we stated, that by the action of acetyl chloride on aurin, we obtained a colourless crystalline compound, which we intended to examine more closely. We have since found that this body is identical with a compound, which Gräbe and Caro[†] obtained by the direct union of aurin and acetic anhydride and having the formula $C_{19}H_{14}O_3+C_4H_6O_3$.

We also mentioned that the purification of this substance was found to be beset with several difficulties. The cause of this was found out after some trouble, but at the same time we were rewarded by the discovery of a series of remarkable bodies, consisting of combinations of aurin with mineral acids.

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[*] Proc. Lit. and Phil. Soc.—1878, 141.

[†] Ber. Deutsch. Chem. Ges.—xi., 1,122.

These salts, as we may call them, are beautiful bodies, crystallizing exceedingly well, and although some of them are decomposed by water, they are very stable in dry air. To their discovery we were led by the following observations.

On heating aurin with glacial acetic acid and acetyl chloride, the crystals lose at once their steel-blue lustre and assume a pale red colour. To obtain the compound thus formed in a pure state, acetyl chloride was added to a saturated solution of aurin in acetic acid. The liquid assumed at once a much lighter colour, and soon pale red needle-shaped crystals having a diamond lustre separated out. On recrystallizing these repeatedly from alcohol, we obtained oblong six-sided plates, which as analysis showed were pure aurin.

On treating the original crystals with water, they become dull and brownish red, the solution containing acetic and hydrochloric acids. It therefore seemed not improbable that an additive product of aurin and acetyl chloride had been formed, containing however also acetic acid, as a superficial examination showed, that the liquid contained, to one molecule of hydrochloric acid, much more than one molecule of acetic acid. We therefore tried to obtain an analogous benzoyl-compound and to determine in it, after decomposition with water the relative quantities of hydrochloric and benzoic acids.

On adding benzoyl chloride to a hot solution of aurin in acetic acid, similar crystals as before were obtained, which, after being dried on filter paper in dry air, were decomposed by water, but only hydrochloric and acetic acid went into solution, and on heating the product with water or alkalies but a mere trace of benzoic acid could be detected.

These facts, coupled with the observation that the bright red needles which, as we stated in our former paper, are formed by crystallising aurin from hot aqueous hydrochloric acid, retain the latter obstinately, led us to the conclusion that this acid forms a definite compound with aurin.

Such a body could be formed under the above conditions, as our glacial acetic acid contained a little water. Moreover, Mr. Charles Lowe had informed us that the splendid specimen of aurin which he exhibited at Paris was obtained in the following way. The crude but crystalline aurin, which is obtained by heating pure phenol with sulphuric and oxalic acids, was dissolved in alcohol and some strong hydrochloric acid added, by which a crystalline precipitate was formed, crystallising from hot acetic acid in beautiful red, glistening, flat needles. He was kind enough to give us a sample, and on examining it we found that water acted upon it in the same way as on our crystals.

In order to prepare a pure compound for analysis, a hot solution of aurin in acetic acid was saturated with hydrochloric acid gas; the colour of the liquid changed into a light yellowish red, and soon the compound separated out in glistening needles, which, even when perfectly dry, smell strongly of acetic acid. When exposed to the air, they soon assume a steel-blue lustre and gradually crumble into a reddish brown crystalline powder. The same properties are shown by the crystals obtained from acetyl chloride and those obtained from Mr. Lowe. When heated to 110° in a current of dry air they gradually lose all the acetic acid, which plays the part of water of crystallisation, and assumes a dull red colour.

On passing hydrochloric acid gas into an alcoholic solution of aurin, similar but smaller needles are formed, containing alcohol, which is given off at 100°; the dull red residue can, like the preceding one, be heated to 190° in a current of dry air without losing hydrochloric acid, which only begins to escape at 200°.

Analysis of these compounds showed that the dried substance consists of $C_{19}H_{14}O_3$, HCl, while the crystals obtained from an acetic acid solution have the composition $C_{19}H_{14}O_3$, $HCl+2C_2H_4O_2$, and those from alcohol $2C_{10}H_{19}O_3$, $HCl+3C_2H_6O$.

When sulphuric acid is added to a hot alcoholic solution of aurin, small red needles are formed on cooling, which consist of $(C_{19}H_{14}O_3)_2\,SO_4H_2+$ Alcohol. Under the same conditions an acetic acid solution yields fine prismatic crystals or flat, very glistening needles, which are an acid sulphate, its formula being $C_{19}H_{14}O_3$, SO_4H_2+ Acetic acid.

We have also prepared a nitrate which is readily formed and crystallizes well but have not analysed it yet.

In our first communication to the Chemical Society, we described a compound of aurin and sulphur dioxide, which is easily obtained in bright red crystals by passing sulphur dioxide into a saturated alcoholic solution of aurin. Our former observation that this body contains water, but no alcohol, we found confirmed; on heating it decomposition easily takes place, pure aurin being left behind, but it appears to be quite stable when exposed to the air, and even on heating it with water no sulphur dioxide is given off, but a drop of sulphuric acid added to the mixture is sufficient to evolve the gas abundantly. Aurin sulphite has the composition $(C_{19}H_{14}O_3)_2SO_3H_2+4H_2O$.

As we have already showed, aurin forms very characteristic compounds with the acid sulphites of the alkali-metals, which in accordance with the newly established formula of aurin must now be written as follows:

$$C_{19}H_{14}O_3,\ SO_3KH$$
$$C_{19}H_{14}O_3,\ SO_3NaH$$
$$C_{19}H_{14}O_3,\ SO_3(NH_4)H$$

We have also found that rosolic acid, or the next higher homologue of aurin forms compounds with mineral acids which crystallize well. Being therefore a base like aurin, we think its name ought to be altered and, as it has only been obtained from rosaniline, propose for it the name *rosaurin*.

"On the Estimation of Small Excesses of Weight by the Balance from the time of Vibration and the angular Deflection of the Beam," by J. H. POYNTING, B.A., B.Sc.

While working last year on an experiment to determine the mean Density of the Earth by the balance, I had to measure such an exceedingly small difference of weight, that I could not at that time estimate it by means of a rider but was obliged to adopt the method described in this paper. Stated generally it consists in treating the balance as a pendulum. Knowing the nature of the pendulum (that is its moment of inertia), and its time of vibration, we can calculate what force acting at the end of one arm of the beam will produce a given angular deflection. It is in fact an application to the common balance of the method which has always been used with the torsion balance when it has been necessary to calculate the forces measured in absolute measure. I cannot find any record of a previous application of the method, and as it might be of use in very delicate weighings, or in verifying the small weights in a laboratory. I have thought it worth while to give a full account of it.

When small quantities of the second order are neglected and the oscillations are of the first order, it will easily be found that the equation of motion of the beam of the balance is

$$\left(MI^2 + \frac{2Pa^2}{g}\right)\ddot{\theta} + (2Ph + Mgk)\theta = ap \dots\dots\dots\dots\dots\dots (1)$$

where MI^2 = moment of inertia of beam about central knife edge
M = mass of beam
a = half length of beam.
P = weight of either pan and the mass in it.
h = distance of line joining terminal knife edges below the central knife edge.
k = distance of centre of gravity of beam below central knife edge.
p = small excess in one pan.

θ = angular deflection in circular measure produced by p.

g = gravity.

If $\ddot{\theta} = \theta$ we have the position of equilibrium given by.

$$\theta = \frac{ap}{2Ph + Mgk} \quad \text{.................................}(2)$$

The semi-periodic time is

$$t = \pi \sqrt{\frac{\frac{MI^2 + 2Pa^2}{g}}{2Ph + Mgk}} \quad \text{.............................}(3)$$

From equations (2) and (3) we can eliminate $2Ph + Mgk$, obtaining

$$p = \pi^2 \frac{MgI^2 + 2Pa^2}{ag} \, \frac{\theta}{t^2} \quad \text{..............................}(4)$$

From this expression it appears that if we know the moment of inertia of the beam, its length, and the weight at each end, we can find the excess p from the time of vibration and deflection.

The results given in this paper were obtained with a 16-inch chemical balance by Oertling. The exact length of the half beam (a) measured by a dividing engine is 20·2484 centimetres.

To find the moment of inertia MI^2 of the beam—The simplest way theoretically would appear to be this. Find the times of vibration t_1 t_2 and the deflections θ_1 θ_2 due to the same excess p with two different loads P_1 P_2 in each pan. Equating the values of p given for each by equation (4) we have

$$\frac{MgI^2 + 2P_1a^2}{MgI^2 + 2P_2a^2} = \frac{\theta_2 t_1^2}{\theta_1 t_2^2}$$

An equation which will give MgI^2 in terms of known quantities. But on trial it was found that a very small proportional error in the observed time made a large error in the value of MgI^2, and the following method, that usually adopted in magnetic observations was employed in preference. A stirrup was suspended by a platinum wire and its time of vibration (t_1) against the force of torsion (μ) of the wire was observed. The moment of inertia of the stirrup being S we have

$$t_1^2 = \frac{\pi^2 S}{\mu}$$

The time of vibration (t_2) was then observed when a cylindrical brass bar of known moment of inertia B was inserted in the stirrup. We now have

$$t_2^2 = \frac{\pi^2 (S + B)}{\mu}$$

The bar was then removed and the balance beam inserted in its place, and the time of vibration (t_3) gives

$$t_3^2 = \frac{\pi^2 (S + MI^2)}{\mu}$$

From these three equations eliminating S and μ we obtain

$$MI^2 = \frac{B(t_3^2 - t_1^2)}{t_2^2 - t_1^2}$$

Now Bg was calculated from the weight and dimensions of the bar to be 6332.83 (in centimetres and grammes). The observed times were $t_1 = 3.6792''$; $t_2 = 4.495''$; $t_3 = 7.1483''$; From these values we find

$$MgI^2 = 35651.6^*$$

To measure θ. The angle of deflection was measured by the number of divisions of the scale which the pointer moved over. As the length of the pointer is 32·1006 centimetres, while 20 divisions of the scale measure 2·5658 centimetres, a tenth of a division, in terms of which the deflection was measured corresponds to an angle of 0·0003996. The oscillations were observed from a distance of 6 or 8 feet by a telescope. The resting point (*i.e.* the point where the balance would be in equilibrium) was found in the usual way by observing the three successive extremities of two swings and taking the mean of the second and the mean of the first and third. Five determinations of the resting point were usually made with the excess to be measured alternately added and removed. From these five, three values of the deflection (n) due to the excess were calculated in a manner which will be seen from the example below.

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* To this a small correction should be added if the adjusting bob is not in its lowest position. This amounts to 7.6 for each turn of the screw and may therefore in general be neglected.

The time of vibration..—This was found from several determinations of the time of ten oscillations. The method will be seen from the example. No correction was needed for the resistance of the air as long as the vibrations did not exceed two divisions of the scale. When however they were much more than that the time of vibration was found to increase with the arc. As the time of vibration frequently changes slightly, probably through variations of temperature. it was usually observed before and after the determination of the deflection (n) and the mean of the two taken as the true time.

The following example of the determination of the value of a centigramme rider by placing it half way along the beam will sufficiently explain the details of the method.

TIME OF VIBRATION AT COMMENCEMENT.

POINTER APPARENTLY MOVING FROM LEFT TO RIGHT.					POINTER APPARENTLY MOVING FROM RIGHT TO LEFT.				
No. of Vibration.	Observed time of passage of pointer through resting point.	No. of Vibration.	Observed time of passage of pointer through resting p't.	Time of 10 Vibrations.	No. of Vibration.	Observed time of passage of pointer through resting point.	No. of Vibration.	Observed time of passage of pointer through resting p't.	Time of 10 Vibrations.
0	11h 15′ 36″	10	17′ 43″	127	1	11h 15′ 49″	11	17′ 56″	127
2	16′ 1″	12	18′ 8″	127	3	16′ 14″	13	18′ 21″	127
4	16′ 25″·5	14	18′ 33″·5	127	5	16′ 39″·5	15	18′ 46″	126·5
6	16′ 52″	16	18′ 59″	127	7	17′ 5″	17	19′ 11″·5	126·5
	Mean value of 10 vibrations..			127		Mean value of 10 vibrations..			126·75

Mean of means = 126·875.

t_1 = 12·6875″

DETERMINATION OF DEFLECTION n.

Excess Weight.	Extremities of Oscillation.	Resting Point.	Mean of preceding and succeeding resting points.	Deflection due to Excess.
Added	109 96 109	102.5		
Removed.........	93 40 92	66.25	102.25	36
Added	152 53 150	102	66.75	35.25
Removed.........	80 55 79	67.25	102.5	35.25
Added	147 60 145	103		

Mean value of n = 35.83.

TIME OF VIBRATION AT END.

POINTER APPARENTLY MOVING FROM LEFT TO RIGHT.					POINTER APPARENTLY MOVING FROM RIGHT TO LEFT.				
No. of Vibration.	Observed time of passing of pointer through resting point.	No. of Vibration.	Observed time of passage of pointer through resting p't.	Time of 10 Vibrations.	No. of Vibration.	Obstrved time of passing of pointer through resting point.	No. of Vibrations.	Observed time of passage of pointer through resting p't.	Time of 10 Vibrations.
0	11h 26′ 19″	10	28′ 27″	128	1	26′ 32·5″	11	28′ 39″	126.5
2	26′ 44″·5	12	28′ 53″	128·5	3	26′ 58″	13	29′ 5″	127
4	27′ 10″	14	29′ 18″	128	5	27′ 23·5″	15	29′ 30″·5	127
6	27′ 35″·5	16	29′ 44″	128·5	7	27′ 49″	17	29′ 56″·5	127·5
	Mean value of 10 vibrations..			128·25		Mean value of 10 vibrations..			127

Mean of Means=127·625.

t_2=12·7625.

Remembering that one tenth of a division of the scale is an angle of ·0003996 in circular measure formula (4), expressed in milligrammes, becomes

$$p = \frac{n}{t^2}\,0{\cdot}3996\frac{\mu^2}{ag}(\mathrm{M}g\mathrm{I}^2 + 2\mathrm{P}a^2)$$

In our present example

$$n = 35{\cdot}83$$

$$t = \frac{t_1 + t_2}{2} = 12{\cdot}725''$$

$$MgI^2 = 35651$$

$$2Pa^2 = 94704^*$$

$$p = 5{\cdot}724 \text{ milligrams.}$$

The length of time occupied in this determination was not quite a quarter of an hour.

The following table contains a series of results which I have obtained of the weight of two centigramme riders, the first of which was accidentally destroyed after the conclusion of the 4th determination. As the rider was always placed at division 5 on the beam, the values given in the table are double those actually obtained.

No. of Experiment.	MgI^2+2Pa^2	t in seconds.	n	Weight of rider in milligrammes.	Mean Value.
1	145364	8·921	13·458	9·78	9·96 mgms.
2	309356	17·65	25·49	10·05	
3	519769	20·435	19·12	9·55	
4	130355	13·10	34·71	10·47	
5	130355	12·87	36·6	11·44	11·35 mgms.
6	130355	12·72	35·50	11·35	
7	130355	12·725	35·83	11·45	
8	130355	12·81	35·5	11·20	
9	130355	12·903	36·37	11·31	
10	454405	19·406	22·08	10·58	

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* For this as for several other cases I removed the pans and hung the weights directly by fine wires from the suspending-pieces. By this means the resistance of the air was very much diminished.

Ordinary Meeting, December 24th, 1878.

J. P. JOULE, D.C.L., LL.D., F.R.S., President, in the Chair.

"Note on the Intensity of Moonlight," by **HARRY GRIMSHAW**, F.C.S.

At the meeting of this Society held on the 26th of November of the present year, some remarks were made upon the relative intensity of moonlight, daylight during a thick fog, and the diffused light of the electric arc.

The opinion was then expressed by some members that it was very doubtful whether it was possible to read printed type of anything like a small size by the brightest moonlight in this country.

The light of the moon on the 8th of the present month (December) being of considerable intensity, I took the opportunity of making the experiment, which I have previously, at different times, performed more roughly, with sufficient accuracy to form some standard of comparison. The result was that I found it possible to read with certainty a still smaller type than I had previously known it possible to peruse.

The types on which I experimented were those of a newspaper, and known technically by the printers as "minion," "bourgeois," and "nonpareil." These are all smaller than the letters of the "Proceedings" of the Society, which I believe is called "small pica." The two first are what are ordinarily used for such printing as that of newspapers and the cheaper periodicals, whilst the last ("nonpareil") is a very small letter indeed. The last paragraph of this note is printed in the type in question. The printed matter in the "minion" and "bourgeois" letter was read with comparative ease; the "nonpareil" had to be perused slowly, but could be made out with certainty.

The time at which the above experiment was performed was 8.15 p.m., and the evening of course bright and cloudless. The moon was full at 7.49 p.m. the following evening. The observers were three in number, who all succeeded in reading the three types, and it should perhaps be added, as a quantity affecting the results, that the "eyes" were all comparatively young, being under the age of thirty.

Ordinary Meeting, January 7th, 1879.

J. P. JOULE, D.C.L., LL.D., F.R.S., President, in the Chair.

"On Boulders of Clay from the Drift," by E. W. BINNEY, F.R.S., F.G.S.

In making the new railway from Manchester through Cheetham Hill to Radcliffe, a fine section of the drift deposits has been exposed in the cutting at Moss Bank, south of the bridge near Crumpsall, at an elevation of 207 feet above the level of the sea. The beds occur in the descending order as follows:

	ft.
1. Clay containing a few pebbles	12
2. Sand with beds of fine gravel	20
3. Clay containing the boulders of clay exposed.	12

The first-named bed would be termed by Professor Hull, F.R.S., upper boulder clay, the second middle sands and gravel, and the third probably lower boulder clay.—See his paper on the Drift deposits in the neighbourhood of Manchester, published in vol. II. (third series) of the Society's Memoirs.

In No. 2, which is composed of fine stratified sand parted by thin beds of gravel and deposits of drifted coal, are found some basin-shaped deposits of gravel about 3 feet long and 1 foot 6 inches deep, which not only contain the pebbles usually found in the till much rounded, but also rounded pieces of till or boulder clay of lenticular and spherical shapes, varying in size from one to three inches through their major axes. The specimens on the table are three taken out of the cutting by myself, and are a fair sample of the average boulders.

There is not a section of the whole of the drift deposits at Moss Bank, but at the Manchester Workhouse, about quarter of a mile distant, the following deposits occur, the second being the equivalent of No. 1 in the preceding section:

	ft.	in.
Clay with a few pebbles	21	0
Quicksand	26	0
Loam	0	4
Clay	30	0
Clay in laminæ	5	0
Clay with stones	4	0
Hard clay	0	6
Sand	2	0
Gravel	7	6
Clay (till)	25	0

Sand with small stones (thickness not given).
Rock (trias).

To my knowledge no instance of boulders of clay in drift beds has been published. Many years since, when, observing the fine cliffs of till at Blackpool, I noticed how the waves rounded the stones from that deposit which fell down on the shore, and I also saw a few of the pieces of clay rounded as well. In the last December number of the Geological Magazine is a description of some clay boulders by Mr. T. Melland Reade, F.G.S., lately observed by him on the Crosby shore near the river Alt. After describing a trench about 50 yards long, 5 feet wide and 2 feet deep, cut in the blue clay which underlies the peat and forest bed,* and which formerly had only a deposit of sand at the bottom, he says "that at the present moment it is filled up nearly to the surface with an agglomeration of rounded lumps of clay more or less compacted together. The clay boulders, for such they are, vary in size from 18 inches on the longer axes to the size of a bean, and from a spherical to an ellipsoid figure."

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* *Quart. Jour. Geol. Soc.*, vol. xxxiv. p. 447.

"We have not far to seek for their origin, as a visit to the lower edge of the peat frayed into a sort of a subtidal cliff or series of cliffs by the encroachments of the sea, shows a deposit of similar clay boulders at its base. In the neighbourhood of the beach the river Alt, meandering over the shore, has made inroads on the post-glacial deposits which compose the substratum, forming a subtidal river cliff of blue clay at its western margin. Lumps of this clay undermined by the currents fall, break up into pieces, and get rolled into boulders by the action of the tide. The trench has formed a sort of trap for catching and retaining them. The clay boulders are in contact, and become in the trench compacted together into one solid mass, so that if it were converted into rock its structure would show in some cases distinct argillaceous boulders in a sandy argillaceous matrix, and in others an imperceptible shading of the boulder nucleus into the matrix."

The boulders of clay found at Moss Bank have in all probability been formed in a similar manner to those described by Mr Reade, as they are found in a hollow in the sand of a basin-shape in vertical section, which would be somewhat like the trench described by him, having fallen from an old cliff of till and been rolled by water into the place where they are now found. In the sand are sometimes found minute fragments of shells, but in such a state that it is impossible to determine what they are.

Ordinary Meeting, January 21st, 1879.

J. P. Joule, D.C.L., LL.D., F.R.S., President, in the Chair.

"On an Old Letter of the late Sir Walter Scott."

E. W. Binney, V.P., F.R.S., F.G.S., said that on March the 16th, 1868, he exhibited at the Society's meeting an original letter of the late Sir Walter Scott, dated the 4th June, 1802, on the subject of an old Scotch Ballad called Jock o' Milk. Since that time he had come into possession of another letter of the baronet's, as well as one from Mr. Liddesdale. They are as follow :—

"Sir,—I am honored with your very obliging favour, and beg leave to express my best thanks for the information which it so handsomely communicates. In the late Mr. Riddell of Glenriddle's MS., which I have frequently referred to in the late compilation, there is a copy of the Ballad called *Jock of Milk*, which I examined very attentively. I was only deterred from publishing it by the strong doubts I entertained of its authenticity, as it appeared to me to bear more the character of an imitation than of a real ancient ballad. It is very possible, however, that I may be mistaken, or that the copy I have seen may be interpolated, and I shall be very much gratified indeed by your furnishing me with the copies which you have so handsomely offered to send me, with as much of the traditionary history as you recollect. I should be also much interested to know whether the verses were taken down from recitation or from a MS., ancient or modern. I have been very desirous as far as possible to ascertain the authenticity of the old poems which I have given to the world, as literary forgeries have been but too often and too justly imputed to the Scottish antiquaries. The Galliard mentioned in your fragment was, I believe, a castle upon the *Seine* belonging to the French monarchs, which gave a name to the favourite dance there practised, just as a more modern dance was called the *Louvre*, and as we call our Highland dance a *Strathspey*. I beg you to believe that I am extremely sensible of your polite attention to the researches of a total stranger, and that I feel myself very much gratified by the interest you have taken in them.

"I have the honor to be, Sir,
"Your obliged and faithful servant,
"WALTER SCOTT."

"Laswade Cottage,
"near Edinburgh,
"2 April, 1802."
"R. Cleator, Esquire,
"Cropton Lodge, near Pickering, Yorkshire."

"East Wood, 9th April, 1802.

"Dear Sir,—I am this morning favoured with your letter of 7th, and lose not a moment in complying with your desire. The old ballad of Jock o' Milk was given to me by Mr. Bell Irving, of Whitehill, and the notes thereon were collected from old tradition, but really not having a copy by me I cannot bring the whole to recollection. Mr. Bell Irving's grandfather, old Whitehill, was many years factor or steward to the family of Castle Milk, and having access to the Repository of all the deeds and papers belonging to that antient place, he found this ballad amongst them, so the present Mr. Bell Irving informs me. From many enquiries amongst very old people now no more, I could perceive there had been such an old ballad, but of which they had a very imperfect idea, but some time it strikes me that two verses are added by the present Mr. Bell Irving. This, however, you can easily detect by writing to him for every particular, and you can then see that if it tallies with what he told me. His direction is W. Bell Irving, Esquire, of Whitehill, near Ecclefechen, N.B. If you have leisure, and could take the trouble to copy the old ballad, with notes, &c., I should then be able to point out something more satisfactory than the above imperfect account is.—Believe me ever

"Yrs truly,
"R. LIDDESDALE."

"William Cleator, Esqre,
"Cropton Cottage, near Pickering."

From the letter formerly published it does not appear that Sir Walter Scott had ever his doubts removed as to the genuineness of the old ballad. The above letters are published in the hope that some reader may possibly take up the subject and clear up the doubts, if this has not already been done.

"On a further Analysis of the Water of the Mineral Spring at Humphrey Head," by C. GRIMSHAW and H. GRIMSHAW, F.C.S.

At a meeting of this Society held on November the 28th, 1876, one of us, in conjunction with Mr. J. Barnes, read a paper on some analyses of this mineral spring, samples having been taken in the month of August in 1875 and 1876 respectively.

The above quoted paper also referred to an analysis of the water of this spring by T. E. Thorpe, F.R.S., in the year 1868. It was noted that during this period the composition of the mineral matter contained in the water had remained remarkably constant during this period of eight years; and it was also proposed to ascertain, after the lapse of another period, whether this constancy was still maintained.

The results of analyses performed with this view on a sample taken on August 16th, 1878, we now beg to lay before the Society. The following are the numbers obtained:

(1) Total solid matter—50 cc. evaporated and dried at 100°—110° C. gave 0·3735 grms.

(2) Chlorine—(*a*) 5 cc. took 16·6 cc. standard silver nitrate (1 cc. = 1 mgrm Cl).
(*b*) 5 cc. took 16·8 cc. standard silver nitrate (1 cc. = 1 mgrm. Cl).

300 cc. of the water gave

(3) Silica	0·004	grms.	SiO_2,
(4) Iron and alumina—equal	0·009	"	Fe_2O_3
(5) CaO—	0·2125	"	CaO
(6) $Mg_2P_2O_7$—0·1665 containing	0·06	"	MgO
(7) PtK_2Cl_6—0·142 "	0·0433	"	KCl

A calculation of these figures gives the following results:

Silica (SiO_2)	0·90	grains per gal.
Iron and alumina, as (Fe_2O_3)	2·10	"
Lime (CaO)	49·58	"
Magnesia (MgO)	14·00	"
Sulphuric acid (SO_3)	76·58	"
Chlorine (Cl)	233·80	"
Alkaline metals and carbonate	145·94	"
	522·90	

A recalculation of the above results on the assumptions as to combination of our previous analyses gives the following:

Silica	0.90	grains per gal.
Iron and alumina	2·10	"
Calcium carbonate	8·40	"
Calcium sulphate	109·72	"
Magnesium sulphate	18·06	"
Magnesium chloride	19·00	"
Potassium chloride	10·11	"
Sodium chloride	354·22	"
	522·51	

By an oversight in our analysis of November, 1876, the potassium chloride is made 84 instead of 8·4, which of course affects the proportions of the two alkalies, but does not alter the relations of the other salts. The potash in the present analysis was determined directly, but in the former by difference from the total alkali and chlorine.

On comparison, the water of this spring in the different years alluded to will be found to contain a very constant amount of mineral matter, which does not vary in composition to any great extent. The respective amounts of different years are as follows:

1868—Thorpe	508·5	grains per gal.
1875—J. B. and H. G.	510·3	„
1876—J. B. and H. G.	514·6	„
1878—C. G. and H. G.	522·5	„

The flow of water on August 16th, 1878, was at the rate of half a gallon per minute. This is just half the rate found by Thorpe in 1868, who during the previous two years had found the flow very constant. A long period of small rainfall occurred just previous to the date of our sample.

We may add, that in a paper read before this Society (Second Series, Vol. XII.) by Mr. Binney, F.R.S., will be found some additional information as to the geological strata of the locality of Humphrey Head, accompanied by a section of the Head and the neighbouring point, which agree with the remarks on this point in our previous paper.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 2nd, 1878.

CHARLES BAILEY, F.L.S., in the chair.

Mr. **PLANT**, F.G.S., stated that a specimen of the Black-throated Wheat-ear (Saxicola stapazina, Temminck.) had been shot at Rawtenstall, near Bury, Lancashire.

Professor W. C. **WILLIAMSON**, F.R.S., remarked that the supposed Radiolarium from the coal measures that he had lately brought before the notice of the Section, had been submitted to Professors Haekel and Strassburger, and pronounced by them both to be a spore.

Mr. **MARCUS M. HARTOG**, F.L.S., read a paper entitled "A Preliminary Abstract of an Investigation of the Nervous System of Cyclops."

Two descriptions exist of the Nervous System of Cyclops; one by Zenker, who describes a chain of ganglia; another by Claus, completed by Leydig, correct so far as it goes, of a brain, an aesophageal ring, and a ventral cord, only seen in the last two thoracic segments, after which it is described as forking. These last observers also describe the antennary nerves as having each branch thickened in its course into a ganglionic ring. I am able to confirm this, and to add the following points:—The ganglionic swellings are found near the terminations of *all* sensory nerve fibres. I have succeeded in seeing the central nerve cord more fully, i.e., from the 2nd thoracic segment onwards. At the end of the 3rd segment it gives off a pair of nerves which pass superficially over the ventral surface of the next segment to the integument. At the end of the 4th segment not only are there a pair of sensory branches given to the rudimentory appendages, but just before a pair are given off to the two large ventral muscles (originating from the sternum of the 5th segment), on which each ends in a beautiful nucleated Doyerian eminence. In the 1st abdominal segment is the fork described by the two German observers, but this takes its origin from the superficial (ventral) aspect of the cord which is continued onwards under the colleterial gland. After running obliquely outwards each branch of the fork subdivides into two, an anterior sensory and a posterior muscular branch. At the commencement of the third abdominal segment the ventral cord forks, its branches diverge slightly in this segment, but more in the next, rising to the sides of the intestine, and having the ventral muscles of this segment superficial to them. In the last segment they have left the intestine and run about the horizontal median plane straight into the axis of either branch of the furca. I have only been able to detect two crystal spheres in the eye of Cyclops; the anterior one in Cyclopsine is here absent.

What seemed to me to be pits with a small mouth so as to form a nearly complete sphere occurring in the rudimentary legs of the last thoracic segment are probably sense organs.

The ventral cord in Cyclopoids contains the elongated nuclei everywhere found on nerve cords, but no ganglion cells; it has lost its significance as a sense organ, and is purely commissural. No transverse commissures are found anywhere, even after its bifurcation. The cells on the sensory nerve fibres suggest a functional displacement outward of the local nerve centres.

Note added January 20th, 1879:

In the above abstract I have referred to a vesicle seated at the base of the fifth segment, and apparently opening externally. Further work has shown to me that this is always seated on the ganglionic enlargement of the nerve that supplies the appendage; that it opens at the ring marking the insertion of the appendage; that in the male it contains one or several irregular brightly refractive bodies, floating freely in its cavity. Hence I regard this organ as an ear, less developed in the female than the male.

At p. 180, vol. 15, of the Proceedings of the Literary and Philosophical Society Mr. PLANT described a "Beetle of good omen from Yucatan," at which time he was unable to give the generic and specific name. This can now be supplied, as a similar specimen has been shown at a meeting of the Entomological Society of London, by Mr. Randolph Clay, from Mexico, and recognised as Zopherus Bremei.

Ordinary Meeting, February 4th, 1879.

E. W. Binney, V.P., F.R.S., F.G.S., in the Chair.

"The Area of the Middle Drifts as determined by their Contents," by Alfred Bell, F.G.S. Communicated by R. D. Darbishire, F.G.S.

The author, in working out the area of the middle drifts, commenced by pointing out the confusion produced by the sands, gravels, and boulder clays of the kingdom being considered, as has been done by some geologists, of the same age. In proving this he has quoted the various forms that characterise the different deposits, showing that in some, as at Wexford, Bridlington, and in Scotland, the fauna underlying the boulder clay contains many arctic forms which are wanting in the sands immediately above. These sands he further traces by their contents over the middle east coast of Ireland, the western and midland counties of England and Wales, the Isle of Man, and the east and south coasts of England. Having enumerated some of the derived rocks, obtained in one pit alone, with numerous crinoids and corals, a brief reference was made to the number and condition of these latter forms, 35 species of which he had found in one collection alone.

Having pointed out the character of the middle glacial marine fauna, he proceeds to show that the land fauna has been subject to the same mutations, the fauna following the climate.

A list of shells, about 140 species, obtained from the drift gravels, forms an appendix to the paper.

Ordinary Meeting, February 18th, 1879.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

James Bottomley, B.A., D.Sc., and Richard S. Dale, B.A., were appointed Auditors of the Treasurer's Accounts.

"On a Chemical Investigation of Japanese Lacquer, or 'Urushi,'" by Sadamu Ishimatsu. Communicated by Professor Roscoe, LL.D., F.R.S.

During a few months last year I had the opportunity of examining roughly into the nature of "Urushi" in the Laboratory of Tokio University.

The specimen of lacquer which I had under my examination was obtained from Kuyemon Nakamuraya, in Tokio, a large lacquer merchant.

It is a milky juice of pale grey colour, and gives out a certain kind of poisonous volatile gas. Some persons are terribly attacked by this poison, producing a great swelling where the acid comes in contact. During my examination in the laboratory one of the apparatus keepers was terribly attacked by this gas, producing ugly swellings all over the face. He told me at the time it was exceedingly itchy. By using the solution of chloride of sodium, carbonate of soda, acetate of lead, &c., he was said to have recovered within a week. This poison acts only on certain persons. I had to work with it for many days, yet never had any attack of the kind nor felt any uneasiness from it.

It has a sweetish characteristic smell and has an irritating taste. It burns with very luminous flame, evolving dense black smoke like oil of turpentine.

It is to a great extent soluble in benzole, ether, absolute alcohol, &c., leaving behind a blackish grey residue in which gum was found.

Lacquer on exposure to the atmosphere rapidly loses its weight and at the same time blackens on its surface; although this loss is different in different specimens, yet on the average of those which I have examined it seems to vary from 25 to 37 per cent.

When the lacquer is exposed to the action of sunlight in hermetically sealed vessels in the atmosphere or in carbonic acid, blackening does not take place, but a large quantity of moisture collects on the sides of the vessel.

The loss of weight in the atmosphere is almost if not entirely due to the escape of water, with a minute quantity of carbonic acid which may be formed by the oxidation of some organic compound existing in the lacquer. The attempt has been made to estimate the relative amounts of carbonic acid and water, yet it was not successful at the time, being too difficult, and it must be left open to some future investigation.

It is by many supposed to be due to the combined action of light and air that the blackening of lacquer in the air takes place; but this seems to be erroneous from the following experiments. First, I made a square box which had a well fitting sliding door and the inside of which was made perfectly black, so that practically no light is admitted to enter. In it was placed a small quantity of lacquer at dark and the door closely shut; on looking at it the next morning it was observed that the lacquer had turned perfectly black, proving that it is not the light that blackens the lacquer.

Second, the bottle in which I kept my lacquer for more than three months during my examination was exposed to the incident light of the laboratory; the surface of the lacquer was turned perfectly black, forming a wall as it were, while those portions which were in contact with the sides of the bottle, which receive as much light as if there were not any glass sides before it, were not at all blackened. This phenomenon is just complementary to the first one, proving that the blackening in the atmosphere is in all probability due to the oxygen of the air, but not the light alone, nor the combined action of light and air.

The lacquer when distilled with water gives a colourless distillate which is slightly acid to test paper, and the attempt has been made to examine the acid, but not successfully on account of too minute quantity of the substance evolved. Distillation by itself and in a current of steam was tried also, but the results in both cases were the same as the first one. Lastly distilled with a small quantity of dilute sulphuric acid, to aid the substance to distil over, into sugar of lead, scarcely any precipitate was obtained.

Lacquer mixes with any kind of fixed oil in all proportions; hence oil is often added as adulteration, but sometimes it is purposely added to increase its mobility.

The specimen of lacquer which I examined consisted of the following three substances:

	I.	II.
Part soluble in absolute alcohol (resin)	58·24	58·23
Gum	6·34	6·30
Residue	2·24	2·30
Moisture and other volatile matter	33·175	33·170
	100·00	100·00

As I have already mentioned, the lacquer loses its weight very rapidly when exposed to the atmosphere. For the above determination I weighed out each time samples from well stoppered bottles, and determined the weight by difference. Then this was treated with absolute alcohol and the filtrate evaporated to small bulk and dried in an air-bath at 100° C. until the weight remained constant. This is put down as the part soluble in alcohol in the above analysis. The residue was then treated with hot water and the filtrate evaporated and dried at 100° and weighed as gum. The residue after the gum had been removed was then washed and dried on weighed filter at 100° C. and weighed as residue. The moisture and other volatile matter were of course determined by the difference.

The estimation of the amount of part soluble in alcohol after the lacquer has been exposed to the sunlight in open vessel for some 20 or 30 days shows that the soluble part increased up to 72·82 per cent. This number when calculated for the substance to have lost 28 per cent moisture and other volatile matter during exposure, then equals to 58·3 per cent, which is nearly equal and practically the same as the analysis previously given, hence there seems to have been no material change in the amount of matter soluble in alcohol. Now the perfectly dried lacquer after finely powdered was dried at 100°, and analysis gave

Part soluble in absolute alcohol	18·07
Gum	3·63
Residue	78·30
	100·00

Altogether from this analysis, the residue being increased, the lacquer seems to have undergone some change, but possibly this is owing to the fact, that the alcohol as well as water seem to have had less complete access to the material.

Thus the "Urushi" consists of three principal constituents, (1) a resinous part soluble in alcohol, (2) gum, and (3) residue. Although there are, in addition to these, water and volatile matter, as they go away sooner or later before it is used, they are not properly called the constituents.

(1.) Part soluble in alcohol (resin) seems to be the principal portion, and has a smell like ordinary lacquer, but it never dries as the original does. It is brownish black, and slightly sticky to the touch. When treated with potash solution it forms a bluish black precipitate, but nothing is obtained on addition of sulphuric acid to the filtrate.

When boiled with hydrochloric acid the resin is transformed into a substance elastic while hot, something like the mass obtained when heated sulphur is dropped into cold water. When boiled with nitric acid, nitrous fumes are given out, and the mass gradually becomes yellow, and finally a beautiful orange coloured mass was obtained. This mass was washed with hot water several times and then treated with absolute alcohol; the mass was to a great extent soluble in the alcohol, leaving behind a small quantity of a yellowish body (which I think to be part not sufficiently acted upon by the acid.) This alcoholic extract forms a beautiful yellow precipitate with both nitrate of silver and acetate of lead. I took a quantity of alcoholic extract, precipitated it with acetate of lead, and the precipitate was thoroughly washed with absolute alcohol and then decomposed by means of dilute sulphuric acid. (It might be better to decompose this salt with sulphuretted hydrogen, but we cannot do so on account of reducing action of this gas.) The mass was dissolved again in absolute alcohol, then separated from sulphate of lead.

Now then this separated alcoholic extract was again precipitated by sugar of lead, and after filtering and washing, the precipitate was dried partially in an air-bath and carried under the receiver of an air-pump and dried over sulphuric acid.

This lead salt exploded when heated. The amount of lead was estimated as oxide by igniting it with nitric acid.

And the salt was subjected to organic combustion. Nitrogen was determined by Dumas' method.

The following numbers were obtained as the mean results:—

Carbon	26·93
Hydrogen	4·11
NO_2	18·44
PbO	47·42
Oxygen	3·10
	100·00

Now I prepared the silver salt of the same and obtained 18·5 per cent of silver as the result. It seems to give no help as to the formula of this body.

As such was the case, I took alcoholic extract of the original lacquer and precipitated it with acetate of lead, and after requisite purification and drying the precipitate was analysed. Lead determined as before. The following is the mean of the results of two experiments which I was only able to try:

Carbon	50·450
Hydrogen	5·705
PbO	3·775
Oxygen	40·070
	100·000

(2.) The gum is soluble in cold as well as in warm water. It has no smell, almost no taste, it has a yellowish or rather brownish colour, and is of a non-crystalline body. It is quite insoluble in alcohol. On subjecting this substance to organic analysis I got the following percentage of hydrogen, carbon, and oxygen:

	I.	II.
Carbon	41·20	41·45
Hydrogen	6·51	6·58
Oxygen	52·29	51·97
	1C0·00	100·00

These analyses yield a formula approximating to the composition of common gum.

(3) The residue is, I think, nothing more than the mixture of cellusose, bark, dust, &c.

In concluding my paper I must say that I am not at all satisfied with my present analyses, but I thought it might be of some interest to some of you from the point that, although the varnished articles from this juice are so celebrated, yet as far as I am aware, this is the first analyses of the kind that has been heretofore attempted, and might be of some use to those who are interested upon this subject.

Mr. **WILLIAM E. A. AXON** called attention to the interesting brochure on the *arbre-à-laque*, by M. Paul Ory, published in Paris in 1875, a copy of which he exbibited. M. Ory was the first to give in a European language (French) an accurate account of the method by which the tree was cultivated and the sap extracted, and his description was illustrated by many woodcuts from Japanese drawings.

"On the Bursting of the Gun on Board the Thunderer," by Professor **OSBORNE REYNOLDS**, F.R.S., Professor of Engineering, Owens College, Manchester.

In the interval which elapsed between the bursting of the gun and the report of the Committee much thought and some trouble has been expended in divining the possible causes which might, under one set of circumstances or another, have led to such a result. It now appears however that different as have been the various suggestions they all resembled each other in one particular, namely, that they were all wrong.

It is to be hoped, however, that all the ingenuity that has been expended will not have been thrown away and that some improvement may result from the pointing out of such numerous defects. That in some respects, such as the increasing twist and the sudden steps or shoulders on the outside of the gun, the present system is defective is shown quite apart from the recent accident; and although it now appears that the moving forward of the shot as the rammer was withdrawn had probably nothing to do with this accident, it cannot be considered satisfactory that this moving forward should be so much the rule as it is shown to have been in the experiments recently undertaken.

Although at first sight it may appear that the fact of the gun having been loaded with two charges of powder and two shot is amply sufficient to explain the bursting, it may not be useless to examine somewhat closely into what would result under such circumstances. The bursting of a 38-ton wrought-iron gun is an experiment of which we should make the most, as we cannot expect to have it often repeated.

From the first accounts of the accident it appeared as though the gun had simply broken in two, like a carrot, at the first step, and that the front half had gone into the sea. Such a failure would not have implied an excess of pressure. It might have been caused by a great end strain, such as would have resulted had the shot jammed when in full career and carried away the fore part of the gun, or it might have resulted from the gradual weakening of the section of the gun at the shoulder owing to the different degrees of expansion immediately before and immediately behind. One or other of these causes appeared to afford the most probable explanation of the phenomena as described in the early accounts. In various subsequent reports, however, it was stated that fragments of the fore part of the gun were blown about in all directions. So that the gun, instead of having simply broken in two, must have burst like a shell in front of the first shoulder. This fact placed the phenomena in an altogether different light. The explosive bursting of the zone of the gun into fragments implied an enormous excess of pressure at this point of the gun.

In order to cause the tube of the gun to burst longitudinally at all would require several times the normal pressure, and the breaking up of the wrought-iron tube into fragments would show that the force was largely in excess of what was necessary to burst it.

After seeing these reports it appeared certain that the gun had been subjected, at the point of rupture, to a pressure enormously excessive, and the question became, whence could such a pressure have arisen? To me it appeared that nothing short of such an action as might, with a detonating fuse, result from the explosion of gun cotton or dynamite would explain the breaking of the gun into fragments. Had the shot become jammed the pressure might have been raised sufficiently to burst the gun, but with pebble powder even this seemed doubtful, and such an action seemed altogether inadequate to explain the breaking of the gun into fragments. It appeared, therefore, that there was but one conclusion to be drawn—there had been something abnormal in the loading. Had the gun been loaded with small grained powder, gun cotton, or dynamite, instead of pebble powder, such a result might have been produced; but then, the gun would, if it had burst, have burst at the breach unless the shot had slipped forward, and that there should have been two accidents appeared highly improbable. Besides, it was necessary to consider what sort of a mistake was most likely to have occurred; and the only possible mistake that could have been made on the spot appeared to be that of double loading.

The fact that if two complete charges were put into the gun, the powder of the second charge would be directly beneath the point of rupture appeared in favour of this, the easiest mistake. But would, supposing the powder to have been pebble powder, the pressure from the two charges have been sufficient to cause the result? At first it seemed to me that even supposing that the second charge had been ignited by the first, which was doubtful, this would not explain the suddenness or magnitude of the pressure. But on further consideration it appeared certain that the second charge would not be ignited by the fire from the first; and it then became clear that in this very fact we should have an amply sufficient explanation of the excessive pressure.

My object in writing this paper is to point out the probability of this explanation, and so, if possible, to induce the authorities to test it. It occurred to me several days before the report of the Committee appeared, and in spite of the improbability of such a mistake as double loading, I could not shake off the conviction that it afforded the true explanation. As I have pointed out, the blowing into fragments of a wrought-iron tube implied an explosive action such as might result from gun cotton or dynamite but which could not be produced by the slow burning of pebble powder. The point to be explained then is how the second charge could be brought into such a condition that it would explode like gun cotton. To understand this it must be remembered that in the usual way the grains of gunpowder burn from their outside only, so that the thicker the grains the longer will be the time occupied in burning, and for the same weight of powder the slower will the gas be given off. The reason why gun cotton is so much more destructive than gunpowder is not that it gives off more gas weight for weight, but that when ignited by a flash it burns so much quicker. If, therefore, by any means the whole mass of gunpowder could be heated up to the firing point at the same instant, so that the grains fired simultaneously inside as well as out, the action of the powder would be as quick or quicker than the gun cotton. And still further, if besides being heated the powder was compressed into a fraction of the space it usually occupies, the gases so confined would be capable of a still greater pressure.

Now if the after cartridge were fired and the forward cartridge were not ignited by the flash, and considering the length and fit of the shot it could hardly have been so ignited, then the after shot would be driven forward closing on to the forward shot and compressing the powder between until the pressure on the forward shot was at least half as great as the pressure of the gases behind the after shot, which would be between 10 and 20 tons on the square inch. Thus the powder would be subjected to a squeeze between the two shot such as would result from a blow. It would be compressed to a fraction of its former volume. The cubes would be crushed into a cake and the work of compression would be sufficient to heat the powder far beyond its point of ignition. Thus the entire mass of powder would be simultaneously ignited in a highly compressed and heated state. The force of such an explosion would be practically unlimited and would be located at the very point at which the gun burst. Hence in such an action we have ample cause for the effect produced.

But it will be asked why does not the same thing happen when a rifle is doubly loaded? It is said that in that case the second cartridge is generally blown out before it ignites, and this may be so, for in the rifle the pressure of the gas on the shot can never exceed above a twentieth part of what it is in the 12-inch gun, and hence in the case of the rifle its pressure may well be insufficient to ignite the powder between the shot.

This view of the action resulting from the firing of powder by percussion appears to me to be one which it would be well worth while to test, for if proved it would completely re-establish confidence in the strength of the guns, which has been somewhat rudely shaken.

Let a 12-inch gun be loaded with a double charge of powder and a double charge of shot, or a shot of double weight, and fired. If, as is probable, the gun does not burst, confidence in the gun will be re-established. Then let it be loaded twice over with the powder between the shot so as to ascertain whether the action of the powder when fired by percussion would not produce an effect similar to that which we are here considering. The destruction of one gun for the purpose of establishing confidence in all the rest would not seem to be an unworthy sacrifice.

PHYSICAL AND MATHEMATICAL SECTION.

January 28th, 1879.

E. W. Binney, F.R.S., F.G.S., in the Chair.

Dr. **Bottomley** called the attention of the Section to an interesting copy of the Principia of Newton. In addition to being an impression of the first edition it contains the autograph of Edmund Halley. It was a present from Halley to the Abbot Nazari.

Nazari was the editor of a scientific journal at Rome from

1668 to 1681. The following is the entry in Halley's handwriting:—

Illustrissimo Dno
Dro Abbati Nazario
Romæ humillime offert
Edm. Halley.

Subsequently the book was in the possession of Dr. Dalton, and its value is enhanced by his autograph. It was considered desirable by the Section that some notice should appear of this interesting book.

Ordinary Meeting, March 4th, 1879.

J. P. **Joule**, D.C.L., LL.D., F.R.S., President, in the Chair.

Professor **Reynolds**, F.R.S., exhibited on behalf of Dr. Roscoe, F.R.S., two samples of steel, one a piece of tungsten steel, the other a piece of crystalline steel presented to Dr. Roscoe with the following letter:—

44, Chorlton St., City,
February 28th, 1879.

Dr. **Roscoe**, B.A. F.R.S.

Dear Sir,

I send by bearer a piece of crystalline steel ingot made here, melted in the usual way and poured into a metallic mould. I consider it a good specimen of its class, in fact the best I ever remember seeing.

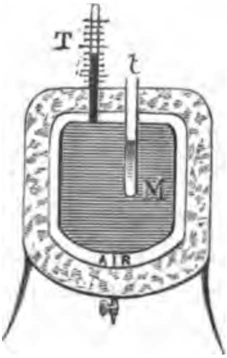
When in this crystalline state it is technically called "struck" metal, and requires great care in re-heating and hammering, or else it comes to grief. I have seen the interesting and varied collection you have for the use of students and send this as an addition to the same, the acceptance of which will oblige,

Yours very respectfully,
WM. ANNABLE.

Professor **Reynolds** also reported that Mr. W. Watts had brought him some stalagmites of ice, of a mushroom shape, quite clear, and about an inch in diameter. They were found at the bottom of the puddle trench at Denshaw, 60 feet deep, and 7 feet broad. These stalagmites showed that the mushroom shape is the result of being formed from spray, and their transparency shows that water spray deposited on ice freezes into solid clear ice and hence confirms the view that hailstones are an aggregation of ice particles.

"On a Modification of Bunsen's Calorimeter," by Professor **Balfour Stewart**, LL.D., F.R.S.

One object of the calorimeter devised by Professor Bunsen is to measure the amount of heat given out by a small body when cooled from the ordinary temperature of the air to 0°C. For this purpose the body is dropped into ice-cold water contained in a small tube which is then closed with a cork to prevent change of air. Now the weight of the body being small compared with that of the ice-cold water into which it is plunged, the temperature of the latter never reaches 4°C. or the point of maximum density of water. The heated water will therefore be specifically heavier than that above it and will remain at the bottom. Nothing can be better than this arrangement for preventing the dissipation of the communicated heat by convection or otherwise, but the method employed by Bunsen of utilizing this heat has proved difficult in practice. It is well known that he makes use of this heat to melt ice contained in a vessel surrounding the bottom of his tube and measures the change of volume produced by the melting of the ice.



I should propose to surround this tube with mercury instead of ice. In the above diagram *t* represents the tube and M a vessel full of mercury into which the tube is tightly inserted, the mouth of the tube extending above this vessel and remaining open at *t* or being closed with a cork. The vessel M is in fact a large thermometer of which the stem and scale are at T. There is an air space which envelopes the vessel M, and finally, around this, we have an outer space filled with melting ice. We may therefore suppose that at the beginning of the experiment the water in the tube *t* as well as the mercury in M will be at the temperature of melting ice. Now let the small body whose specific heat we wish to determine be dropped into the tube *t*. The heat which it carries with it will remain at the bottom of this tube and will be spent in very slightly heating the large mass of mercury in M. This mercury being surrounded with melting ice will receive no heat from any other quarter. Its expansion will therefore be a measure of the heat communicated to the tube *t*, and although the whole rise of temperature of the mercury will not be great yet the mass being large it will act as a very open thermometer and the heating effect will be indicated by a rise of mercury in the stem T.

"The Poisonous Qualities of the Yew," by WILLIAM E. A. AXON, M.R.S.L., F.S.S.

At our meeting, on the 9th Jan., 1877, Dr. Bottomley made an interesting communication to the Society on the real or supposed toxic qualities of yew leaves. The subject recurred to my mind when on a recent visit to Stonyhurst, for the failure of the main line of the Sherburnes, the ancient owners of that mansion, is attributed to a similar cause. Richard Francis, son of Sir Nicholas Sherburne of Stonyhurst, died in 1702, at the age of nine, through eating some yew tree berries, in the fine avenue at the eastern end of that hall.* This tradition led me to make further inquiries. I was informed by Mr. Thomas Kirk, farmer, of Whittingham, near Preston, that he has known cases of the poisoning of cows and other cattle by yew leaves. In the spring of last year a young horse came to an untimely end from the same cause. The poisonous qualities of the yew tree has in that district at least passed into a commonplace of agricultural belief. It then occurred to me that I had read something on the subject in our older botanical writers, and on turning up the fine old folio of Gerarde's Herbal, enlarged by Johnson, and published 1636, I found this passage :—"The yew tree, as Galen reporteth, is of a venomous quality, and against man's nature. Dioscorides writeth, and generally all that heretofore have dealt in the faculty of herbes, that the yew tree is very venomous to be taken inwardly, and that if any doe sleepe under the shadow thereof it causeth sicknesse, and oftentimes death. Moreover, they say that the fruit thereof being eaten is not only dangerous and deadly unto man, but if birds doe eat thereof, it causeth them to cast their feathers, and many times to die. All of which I dare boldly affirme is altogether untrue; for, when I was young, and went to schoole, divers of my schoole-fellows, and likewise myself did eat our fills of the berries of this tree, and have not only slept under the shadow thereof, but among the branches also, without any hurt at all, and that not at one time, but many times." He adds on the authority of Theophrastus that "labouring beasts die, if they do eate of the leaves; but, such cattell as chew their cud receive no hurt at all thereby. * * Pena and Lobel also observed that which our author here affirmes, and dayly experience shewes it to be true, that the yew tree in England is not poysonous, yet divers affirme, that in Province in France, and in most hot countries, it hath such a maligne quality, that it is not safe to sleepe or long to rest under the shadow thereof."*

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* Hewitson's Stonyhurst College, 1878, p. 9.

A writer in Hardwicke's Science Gossip, May, 1865, states that thirty or forty deer in the park of the Duke of Beaufort, at Badminton, were poisoned by nibbling the leaves of a yew tree, the branches of it having borne down within their reach by the weight of heavy snow. A lady (Helen Watney), writing in July, points out that this is contrary to the idea that the leaves are not poisonous to deer, sheep, or goats. She mentions as a well-known fact, that leaves, whether fresh or withered, are toxic in their influence upon horses. Its juice, she adds, was considered a remedy for the bite of a viper, and that in Germany, it is still a popular remedy for hydrophobia. Dr. Taylor cites a case of a child having been poisoned by eating yew berries. Death appears to have ensued in a few hours. He names also a case of a lunatic who died from eating yew leaves. "There is no doubt," he says, "that the yew is a cerebro-spinal poison. The symptoms produced by the leaves and berries are pretty uniform in character: convulsions, insensibility, coma, dilated pupils, pale countenance, small pulse, and cold extremities are the most prominent. In two cases, the subject of one—a girl about five years of age—died in a comatose state in four hours after she had eaten the berries, and the other a boy, æt. four years, died nineteen days after taking the berries, obviously from severe inflammation of the bowels."*

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* Gerarde's Herbal, enlarged by Johnson, 1636, p. 1370.

Dr. Taylor adds that "the nature of the poisonous principle is unknown, and it is not certain whether, with respect to the berry, the poison is lodged in the pulp or the seed, although it is most probable in the latter."

It may be exhibited in another form, for Cæsar, in the Commentaries on the Gallic War, tells us that Cativolcus, King of the Eburnones, unable to endure the fatigue either of flight or of war, poisoned himself with the juice of the yew tree, of which there is great abundance in Gaul and Germany. (B. G. vi., c. xxxi.)

* Taylor on Poisons, 2nd edition, 1859, p. 843.

The poets share the popular belief. Father Newman in a recently published poem, speaks of

.........some dark, lonely, evil-natured yew,
Whose poisonous fruit—so fabling poets speak—
Beneath the moon's pale gleam the midnight hag doth seek.*

M. Maviné has found a poisonous alkaloid in the leaves and seed of the common yew *(Taxus baccata),* which is named *Taxine.* It is nitrogenous, and evolves ammonia when ignited with freshly-ignited soda-lime. Taxine is present in greater quantities in the leaves than in the seeds.—(*Science Gossip,* 1877, p. 141.)

It will be seen that there is considerable diversity in the evidence as to the reality of the poisonous qualities of the yew, both as to its leaves and berries. It seemed worth while to bring this fact out in the hope of inducing some chemist to turn his attention to a matter which still appears to invite definite and decisive investigation. We all know Pascal's famous saying, that if Cleopatra's nose had been an inch longer or shorter, the entire course of subsequent history might have been different. As a smaller example may be mentioned, that popular belief at least regards a childish feast on fatal yew leaves, as one of the main links in that chain of events which has given Lancashire the famous Jesuit College of Stonyhurst.

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* Lyrics of Light and Life, edited by F. G. Lee, second edition, 1878, p. 8.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

January 8th, 1879.

Charles Bailey, F.L.S., President of the Section, in the Chair.

Mr. Hastings Charles Dent, of the City Surveyor's Office, Town Hall, and 112, Bury New Road, Manchester, was elected a member of the Section.

Mr. Thomas Rogers read a paper on, and exhibited many specimens of, ballast plants collected at Cardiff in September, 1878.

The following list shows that the majority of the plants collected are of South European origin, interspersed with a few North American and Eastern Indian species.

Foreign Plants—Cardiff Ballast.

Natural Order.		Native Country. *London Encyc.*
Ranunculaceæ	Nigella damascena	South Europe
Cruciferæ	Berteroa incana	,, ,,
	Camelina fœtida	,, ,,
	Bunias orientale	Levant
	Sisymbrium pannonicum	Hungary
	Iberis umbellata	probably an escape from gardens
	Erysimum orientale	Levant
Resedaceæ	Reseda Duriæi	Algiers
Malvaceæ	Lavatera punctata	Italy
	Malva nicœensis	,,
Limnanthaceæ	Limnanthes sulphurea	California
Zygophyllaceæ	Tribulus terrestris	South Europe
Leguminosæ	Anthrolobium scorpioides	,, ,,
	Cicer arietinum	,, ,,
	Coronilla varia	Europe

	Scorpiurus sub villosus	Europe
	Medicago littoralis	Italy
	Melilotus cœrulea	Switzerland
	Astragalus hamosus	Spain
	Trifolium nigrescens	Italy
	Melilotus alba	„
	Hedysarum coronarium	„
Caryophyllaceæ	Saponaria vaccaria	Germany
Onagraceæ	Clarkia pulchella	North America
	Œnothera biennis	„ „
	„ odorata	South America
Compositæ	Scolymus hispanicus	Spain
	Nardosmia fragrans	Italy
	Inula viscosa	South Europe
	Centaurea melitensis	Malta
	Xanthium spinosum	South Europe
	Erigeron Canadense	North America
	Actinomeris squarrosa ?	„ „
	Solidago lanceolata	„ „
Scrophulariaceæ	Scrophularia canina var.	South Europe
	„ green flowers ?	
	Linaria elatine var. Sieberi	Candia
Labiatæ	Sideritis montana	Austria
	Salvia verticellata	Germany
	Stachys Italica	Italy
Boraginaceæ	Heliotropium europeum	Europe
	Echinospermum Lappula	„
Plantaginaceæ	Plautago arenaria	Hungary
Amaranthaceæ	Amaranthus chlorostachys	South Europe
	„ retroflexus	North America
Chenopodaceæ	Chenopodium ambrosiodes	South Europe
	„ Botrys	„ „
Euphorbiacæ	Euphorbia segetalis	„ „
Urticaceæ	Cannabis sativa	India
	Narcissus Tazetta	Spain
Gramineæ	Crypsis schœnoides	South Europe
	Zea mays	India

Panicum miliaceum	East Indies
Lepturus incurvatus	
Ægilops ovata	South Europe
Alopecurus utriculatus	„ „
Phalaris paradoxa	Levant
„ cærulescens	Spain

The following British plants occurred on the Cardiff Ballast Heaps.

British Plants.

Papaver somniferum and P. argemone.
Lepidium Draba and L. ruderale.
Sinapis nigra.
Senebiera didyma and S. coronopus.
Nasturtium terrestre.
Erysimum Cheiranthoides.
Sisymbrium Sophia.
Arenaria serpyllifolia, var. leptoclados.
Cerastium arvense.
Herniaria glabra and H. ciliata.
Polycarpon tetraphyllum.
Reseda alba.
Lavatera arborea.
Lavatera sylvestris.
Linum usitatissimum.
Geranium rotundifolium.
Melilotus vulgaris.
Trifolium fragiferum, and T. resupinatum.
Trifolium suffocatum, and T. stellatum.
Trifolium incarnatum, and T. Ochroleucum.
Trifolium maritimum.
Medicago falcata and M. denticulata.
Vicia lutea.
Lathyrus aphaca.
Lythrum hyssopifolium.
Epilobium.
Coriandrum sativum.

Eryngium campestre.
Bupleurum rotundifolium.
Centranthus ruber.
Cnicus marianus.
Gnaphalium luteo-album.
Artemisia campestris.
Solanum nigrum, in large bushes.
Datura stramonium.
Hyoscyamus niger.
Senecio squalidus.
Lactuca virosa.
Anthemis arvensis.
Veronica Buxbaumii.
Linaria spuria.
Linaria elatine and var. Sieberi.
Scrophularia Scorodonia.
Salvia clandestina.
Chenopodium rubrum.
Cynodon Dactylon.
Digitaria humifusa and D. sanguinalis.
Panicum crus-galli.
Setaria viridis and S. glauca.
Gastridium lendigerum.
Polypogon Monspeliensis.
Agrostis spica-venti.
Agrostis alba, var. stolonifera.
Lolium temulentum.
Poa loliacea.

General Meeting, March 18th, 1879.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

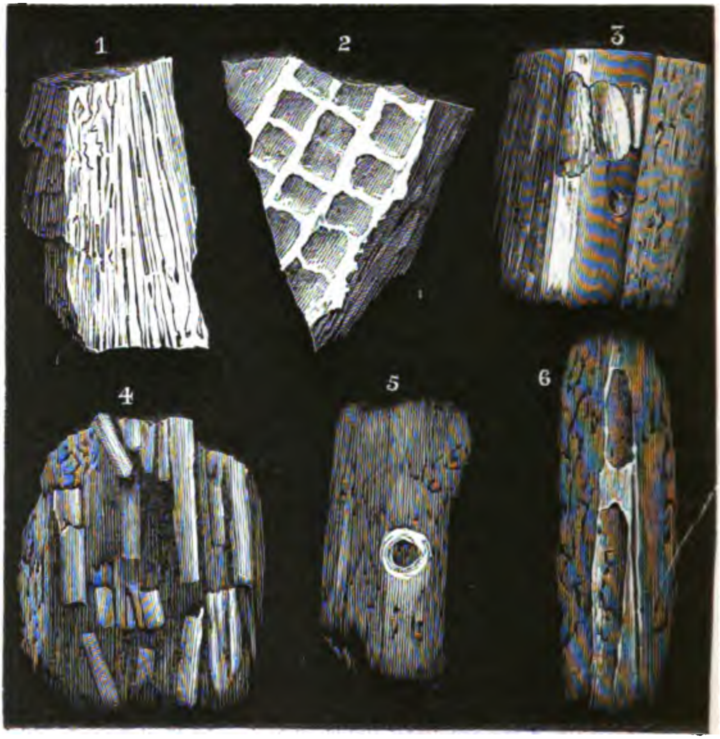
Mr. Hastings Charles Dent, 112, Bury New Road, Manchester, was elected an Ordinary Member of the Society; and Monsieur A. Letourneux, Conseiller à la Cour d'Appel d'Alexandrie, Egypte, was elected a Corresponding Member.

Ordinary Meeting, March 18th, 1879.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"On Siliceous Fossilization, Part II.," by J. B. HANNAY, F.R.S.E., F.C.S., Assistant Lecturer on Chemistry in the Owens College.

In a former paper it was shown by chemical and optical means that the fossil siliceous rods Hyalonema Smithii were identical in constitution with those from modern sponges, and that the curious nodulised appearance of some of the rods was due not to the original form of the rods, but to certain physical and chemical changes which have passed over them since they were deposited where they were found. It was also shown that of the three forms of silica, transparent, gelatinous, and opaque, the first and second were easily acted upon, and retained the original structure of the organic silica, whereas the last was in the truly mineral form and had lost every trace of organic structure, and was not easily acted upon by chemical means. Mr. John Young, F.G.S., having kindly supplied me with several specimens of the fossils from the limestone quarry at Kilwinning, which are very different from those I previously examined, and which throw more light on the changes which siliceous fossils may undergo, I beg to give an account of them to the Society. One piece of limestone simply contained (instead of rods) a number of cylindrical holes where the rods had lain. In Fig. 1, which is a woodcut from a photograph, are seen these cylindrical holes which plainly show that the rods have been dissolved away. The solvent must have been a strong calcareous or other alkaline solution, as the calcareous fossils are not in the least disfigured. In. Fig. 2 we see a beautifully preserved sample of Fenestella, and we know that a very slight



solvent action would have destroyed the structure of these delicate organisms. From other internal evidence, such as little shells and diatomaceæ, it is clear that the calcareous portion of this limestone has not been at any time dissolved to any extent, and yet such an obdurate substance as silica has been completely removed. Then as to the cause of its removal. The solution was no doubt highly calcareous, but we know that highly calcareous water may run over quartz crystals for a very long period without having the slightest effect upon the faces. I think that Fig. 3 will explain how the rods came to be so easily dissolved. This is a photograph of a hollow where a rod has lain which still contains some rounded nodules of silica. It will be seen from my former paper that these nodules are anhydrous inorganic silica, crystallizing out of the hydrated silica after the rod has undergone a little dehydration since it was alive. Now here we see the whole rod dissolved except such portions as were entirely mineralised, if I may use such a term, so that we see that the reason these silica rods were so easily dissolved by the calcareous solution was because the silica of which they were composed was in a hydrated easily soluble form. Thus the existence of those nodules which had before puzzled naturalists now gives us the clue to the state of the rods at the time of solution. Fig. 4 will still further elucidate this subject. Here it will be seen we have a large number of rods partially dissolved. I have examined by the means given in my former paper above twenty samples of these partially dissolved rods, and I have not found one sample containing water, so that again we see the whole of the thoroughly mineralised silica is left behind, and those portions which were very probably hydrated dissolved. I say very probably, for we see that the solvent action has gone on in a very irregular manner and in a manner which could not be accounted for on any circulation hypothesis, but just in such a manner as would be caused by the irregular manner in which the rods get mineralised.

It might be expected that since the rods were so neatly and perfectly dissolved out, the spaces might get filled up with carbonate of lime and reproduce the silica rod as a calcareous fossil; but although I have examined some hundreds of those fossils, I have not found one case of this nature. The only cases I have found even approaching to this is when the centre of the rod is dissolved it is sometimes, as in Fig. 5, filled in with carbonate of lime. I have noticed that when carbonate of lime is deposited in the cavity where the rod lay it is highly crystalline, and could never be mistaken for anything organic. Fig. 6 is from a photograph, showing the carbonate of lime in the silica centre.

As the above remarks border on a subject which has been discussed very extensively, I may be allowed to point out that they settle one half of the discussion, namely, that silica may be dissolved in presence of calcareous fossils; but the other half, namely, whether or not the spaces so left may be filled up with carbonate of lime so as to look like fossils, is still an open question.

PHYSICAL AND MATHEMATICAL SECTION.

February 25th, 1879.

E. W. BINNEY, F.R.S., F.G.S., in the Chair.

"On the Mean Temperatures of the Winters of the last 29 Years," by the Rev. THOMAS MACKERETH, F.R.A.S., &c.

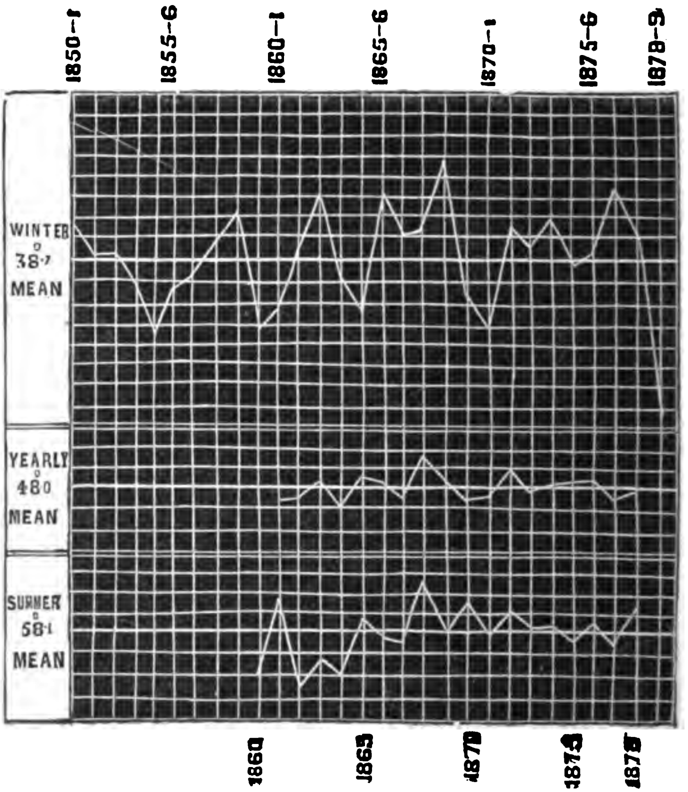
It may be considered somewhat premature to institute a comparison of the mean temperatures of the last 29 years when the colder of the extreme of the winter months has not yet been entered upon, viz., the month of March. In a meteorological sense winter may be considered as including five months of the year, viz., November, December, January, February, and March. But January, so far as low temperature is concerned, is the pivotal month of the winter, and March has a mean temperature slightly below that of November. The difference is so small, only about 0·7 deg. Fahr., that in comparing the mean temperature of winters the mean temperature of these two months may be practically neglected. The mean temperatures deduced from my own observations extend over only 17 or 18 years; but the late G. V. Vernon, Esq., F.R.A.S., was kind enough to furnish me with the weekly temperatures he had deduced from the year 1850 onwards till 1861, when I began to make my own observations and deductions. That the mean temperatures here presented may have a common basis I have calculated them upon the weekly mean temperatures of the last 29 years, which of course include those of the late Mr. Vernon.

The results are as follows for the winters extending from the first week of December in one year to the last completed week in February of the year following.

Winter of	Mean Temp.	Winter of	Mean Temp.
1850-1	40·3°	1865-6	41·9°
1851-2	39·8°	1866-7	39·9°
1852-3	39·9°	1867-8	40·0°
1853-4	37·3°	1868-9	43·6°
1854-5	35·1°	1869-70	37·1°
1855-6	37·3°	1870-1	35·6°
1856-7	37·9°	1871-2	40·3°
1857-8	39·4°	1872-3	39·2°
1858-9	41·0°	1873-4	40·6°
1859-60	35·4°	1874-5	38·6°
1860-1	36·4°	1875-6	38·8°
1861-2	39·3°	1876-7	42·3°
1862-3	41·9°	1877-8	39·6°
1863-4	37·9°	1878-9	31·4°
1864-5	36·6°		

From the above table of winter mean temperatures it will be seen that the coldest winters of the last 29 years were in 1854-55, 1859-60, 1870-71, and in 1868-69; and that the present winter is colder than the coldest of these, viz. 1854-55 by 3·7 deg. or 12 per cent. The mean temperature of the winter months for 29 years is 38·7 deg. And whilst the winter of 1854-55 was 3·6 deg. or a little over 9 per cent below the average temperature, the present winter is 7·3 deg. or 19 per cent below the average winter temperature.

The accompanying diagram sets forth the ratio of all the winters from 1850-51.



Here it will be seen that five years elapsed between the cold winters of 1854-55, and 1859-60; but that six years elapsed between the cold winters of 1864-65, and 1870-71, and that eight years elapsed between the present cold winter and the previous one of 1870-71. It is now known that the sun spot period is irregular and not so nearly an interval of 10, 11, or 12 years as was imagined. The minimum of the sun spot period happened about two years ago, but it is still at a minimum, and very seldom have spots been seen on his disc during the past year. Whether these cold winters are traceable to this solar inactivity or not, the present coincidence is very striking.

In the same diagram I have presented the ratios of the mean summer temperatures of the last 18 years. These ranges are included in the weekly mean temperatures of June, July, and August, and will be seen to be far less than the ranges of the winter mean temperatures. This perhaps may be accounted for by the relative difference of the amount of atmospheric vapour existing in the air in the two opposite seasons of the year. In the winter season the ratio of atmospheric vapour reaches 87 per cent, whilst in the summer season it reaches only about 75 per cent. But whether this is so or not, there is the fact. Here it will be seen that between the hot summer of 1861 and the still hotter one of 1868 there is an interval of seven years; since then the coming summer will make an interval of 11 years. In the interval of 7 years there was only one summer with a temperature slightly above the average, 1865; all the other summers of this interval were mostly far below the average; but in the 11 years' interval all the summers, with only two exceptions, have either been above or equal to the average summer temperature. The ratios of the summer and winter temperatures have not the slightest relationship to each other. Hence it is impossible to form any idea from the temperature of a winter what kind of summer temperature may follow.

I have shown on the same diagram the mean temperature of each year from 1861 to 1878. The mean of all is 48·0 deg. The ratios of their differences are extremely small, so small indeed as to be of no practical value; for between the coldest year and the warmest of the last 17 years there is a difference of only 2·9 deg., and the mean difference of temperature of all these years amounts to only 0·3 deg. These differences do not, from the data I have, seem to observe any definite rule.

Ordinary Meeting, April 1st, 1879.

EDWARD SCHUNCK, Ph.D., F.R.S., &c., Vice-President, in the Chair.

Among the Donations announced was a Portrait of Mr. E. W. Binney, F.R.S., &c., painted by Mr. William Herbert, Johnston, and presented by the President, Dr. J. P. Joule, F.R.S., &c.

On the motion of Dr. R. Angus Smith, F.R.S., seconded by Professor W. C. Williamson, F.R.S., it was resolved That the cordial thanks of the Society be given to Dr. Joule for his valuable donation.

Mr. J. H. POYNTING, B.A., exhibited an autograph dedication to M. Humbolt, of "Extrait d'un mémoire sur la Théorie des tubes capillaires," by M. Laplace, of which the following is a copy:

Pour Monsieur Humbolt. De la part de l'auteur.

Je prie Monsieur Humbolt d'agréer ce petit mémoire comme un hommage de tous mes sentimens d'estime et d'attachement bien sincères. M. Berthollet, qui est avec moi dans ce moment, me prie d'y joindre les siens.

Nous nous rappellons l'un et l'autre au souvenir de M. Gay Lussac.

"Two Deductions from Mr. G. H. Darwin's Letter in *Nature*, Feb. 6, 1879, upon Sir W. Thomson's Equation of Cooling,

$$v = V_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{kt}}} e^{-z^2} dz$$

by Mr. JAMES HEELIS.

If the expression $\int_0^{\frac{x}{2\sqrt{kt}}} \epsilon^{-z^2} dy$ be integrated we get

$$v = V_0 + \frac{2V}{\sqrt{\pi}} z\epsilon^{-z^2} \left\{ 1 + \frac{2}{1\cdot3}z^2 + \frac{2^2}{1\cdot3}z^4 + \frac{2^3}{1\cdot3.5\cdot7}z^6 + \&c. \right\}$$

where $z = \frac{x}{2\sqrt{kt}}\cdot$ Taking Mr. Darwin's value for y in order that it may give the point of maximum cooling of the mass at any given time reckoned from the commencement of cooling ($z^2 = \frac{1}{2}$ or $x^2 = 2kt$) the above becomes

$$v = V_0 - \frac{2V}{\sqrt{2\pi\epsilon}}\left(1 + \frac{1}{1\cdot3} + \frac{1}{1\cdot3\cdot5} - \frac{1}{1\cdot3\cdot5\cdot7} + \&c.\right)$$

$$= V_0 + \frac{2\cdot8213722V}{\sqrt{2 \times 2\cdot7182818 \times 3\cdot1415926}}$$

$$= V_0 + \cdot6826894V \text{ or putting } V_0 = \frac{V_1 + V_2}{2} V = \frac{V_1 - V_2}{2}$$

$$= \cdot8413447V_1 + \cdot1586553V_2.$$

where $V_1 V_2$ are the initial temperatures at commencement of cooling, a very surprising result, showing that the temperature of the point of maximum cooling of the mass is constant and that if Mr. Darwin's assumption as to time is to be admitted, at about 100 miles below the earth's surface there is a temperature $\frac{6}{7}$ths of what pervaded the mass when cooling commenced.

Mr. Darwin's deduced relation $x^2 = 2kt$ must be taken with a qualification. It will give the plane on which the maximum cooling of the mass is taking place at any time, but it will not give the time at which the maximum cooling takes place in any plane.

To show this take the equation which he deduces for the rate of cooling, viz.

$$-\frac{dv}{dt} = \frac{V}{2\sqrt{\pi k}} \frac{x}{t^{\frac{3}{2}}} \epsilon^{-\frac{x^2}{4kt}}$$

$$\log\left(-\frac{dv}{dt}\right) = \log C + \log x - \tfrac{3}{2}\log t - \frac{x^2}{4kt}$$

Differentiate with respect to x, supposing t an arbitrary constant,

$$\frac{1}{-\frac{dv}{dt}}\frac{d\log\left(-\frac{dv}{dt}\right)}{dz}=\frac{1}{x}-\frac{x}{2kt}$$

which equated to 0 gives Mr. Darwin's relation $x^2=2kt$. But if you assume x to be an arbitrary constant and differentiate with respect to t, you get

$$\frac{1}{-\frac{dv}{dt}}\cdot\frac{d\log\left(-\frac{dv}{dt}\right)}{dt}=-\frac{3}{2t}+\frac{x^2}{4kt^2}$$

which equated to 0 gives $t=\frac{x^2}{6k}$.

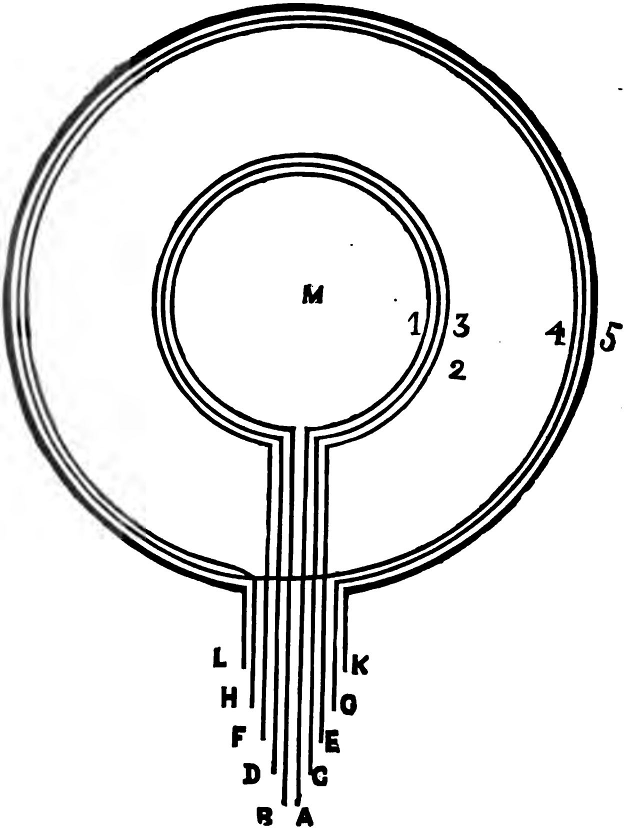
These two relations between x and t show that in any plane the maximum rate of cooling takes place in one third of the time (measured from the commencement of the cooling) that the maximum rate of cooling of the mass takes place in that plane.

The conclusion from this appears to be that if it be admitted that the contraction is proportionate to the cooling, the principal contraction in any plane will take place long before that place is the seat of the maximum cooling of the mass.

I ought to have stated above that both $x^2=2kt$ and $x^2=6kt$ give negative values for the second differential coefficient. That is, of course they do give maxima and not minima.

"Arrangement of a Tangent Galvanometer for lecture-room purposes to illustrate the laws of the action of currents on magnets, and of the resistance of wires," by J. H. Poynting, B.A., Demonstrator in the Physical Laboratory of the Owens College.

Three coils of similar wire are arranged round the circumference of a circle with a compass needle at the centre, each wire going only once round the circle. The six ends of the three wires are connected by thick copper wires with the six binding screws AB,CD,EF.



On a concentric circle of twice the radius is arranged a coil of the same wire going twice round the circle and having its ends connected by thick wires with the binding screws GH. On the same circle is a single wire of twice the diameter, making only one turn round the circle, and having its ends connected with the binding screws KL.

The coils are denoted respectively by the numbers 1—5.

The null method is adopted in each case. That is two forces acting on the needle and arising from different arrangements of the circuit are shown to balance each other, and from the arrangements necessary to produce this equilibrium the desired laws are deduced.

I. *If the current be reversed the force is reversed.* Introduce the current at A. Join BD and lead away at C. Then the same current goes in opposite directions round the coils (1) and (2), and since the needle is not deflected the reversal of the force when the current is reversed is proved.

II. *The force is proportional to the length of current acting.* This is proved by the last, for the two coils (1) and (2) exert equal forces on the needle. If the current went round them in the same direction we should have twice the force which each exerted singly, with twice the length of current. This assumes that the current in different parts of the circuit is the same, which might be shown by slightly modifying I, thus, introduce between B and D various resistances and the equilibrium is not disturbed.

III. *The force is proportional to the strength of the current.* Introduce at A and connect A with C and B with D, and connect D with F and lead off from E. There will be two equal currents in the same directions in coils (1) and (2), for they are exactly similar to each other and similarly situated. These two currents unite to give a double current in the opposite direction in coil (3), and the double current in a single wire exerts twice the force exerted by each single current, since there is no deflection.

IV. *The force is inversely proportional to the square of the distance.* Introduce at A, connect B with H, and lead off at G. Then since we have two turns to the coil (4) we have a current of four times the length at twice the distance acting in opposition to the same current through (1). Since there is no deflection, the two exert equal forces, and therefore the force is inversely proportional to the square of the distance.

RESISTANCE.

I. *The resistance is proportional to the length.* Connect B with C, D with E, A with F. Introduce at A and lead off at E. We have then a divided circuit joining A and E, one consisting of the two coils (1) and (2), and the other the coil (3) only one half the length and going in the opposite direction. But as there is no deflection the current through the first circuit of twice the length must be only one half that through the other to exert an equal force, *i.e.*, the resistance is doubled when the length is doubled.

II. *The resistance is inversely proportional to the cross section.* Introduce at A, connect A with L, connect B with C and D with K, and lead off at K. Then we have two circuits connecting A with K, the first consisting of the two coils (1) and (2), the second of the coil (5) of the same length but of four times the cross section and going round in the opposite direction. Since the needle is not deflected the two currents exert equal forces. But the coil (5) is at twice the distance and must therefore have four times as great a current through it as that through (1) and (2). That is, with equal lengths of wire of the same material connecting two points the currents conveyed are proportional to the cross sections.

"On Aurin, Part II.," by R. S. DALE, B.A., and C. SCHORLEMMER, F.R.S.

Some time ago we read a paper before this Society, in which we stated that, on heating aurin $C_{19}H_{14}O_3$ with an excess of aqueous ammonia, it is converted into pararosaniline $C_{19}H_{17}N_3$.* There ought to exist two compounds, standing intermediate between aurin and rosaniline, viz., $C_{19}H_{15}NO_2$ and $C_{19}H_{16}N_2O$, as the action of ammonia probably proceeds in three stages:

$$C_{19}H_{14}O_3 + NH_3 = C_{19}H_{15}NO_2 + H_2O$$
$$C_{19}H_{15}NO_2 + NH_3 = C_{19}H_{16}N_2O + H_2O$$
$$C_{19}H_{16}N_2O + NH_3 = C_{19}H_{17}N_3 + H_2O$$

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* Mem. Lit. Phil. Soc. (3) vi., 248.

We have made a very large number of experiments for the purpose to get hold of one of these intermediate products, and have at last succeeded in obtaining one, probably the first, in beautiful crystals; it dyes on silk and wool a rich red, and as the commercial red aurin, which is obtained by the action of ammonia on common or yellow aurin, has also been called *peonin*, we will retain this name for our compound. It is formed by heating aurin with dilute ammonia to 100°C for some weeks; a body dyeing exactly the same shade, and therefore undoubtedly the same compound, is obtained by passing for a few hours ammonia gas into a solution of aurin in boiling amyl alcohol.

As we encountered many difficulties in the preparation of peonin, we thought more definite results might be obtained by using a compound ammonia, and therefore tried the action of methylamine on aurin. Methylamine is now a commercial article, being obtained from beetroot-molasses by M. Vincent's method, which Dr. Roscoe described to this society some weeks ago; he was kind enough to present it with the methylamine required for our experiments.

On heating the two bodies together for a few hours, the deep red colour of the liquid became much paler and a brown deposit was formed in the tubes, which on examination was found to be *trimethyl-pararosaniline*, which is formed according to the equation:

$$C_{19}H_{14}O_3 + 3(CH_3)NH_2 = C_{19}H_{14}(CH_3)_3N_3 + 3H_2O.$$

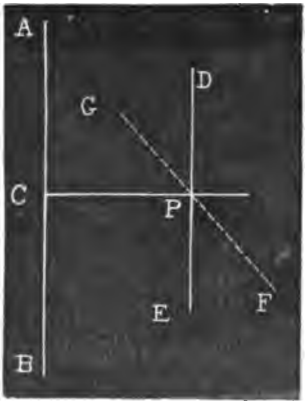
We have dyed with it, as well as with the other bodies mentioned in this paper, silk, wool, and mordanted cotton, and beg to submit these samples to your inspection.

We have already proved that by acting on aurin with aniline, the final product consists of triphenyl-pararosaniline and thus shown, that the whole host of bodies, known as the aniline colours, can be obtained from phenol.

"Screw Propulsion," by **Robert Rawson**, Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society, Member of the Mathematical Society.

Preliminary Proposition.

1. Suppose AB in the annexed diagram to be the axis of the screw propeller; draw CP perpendicular and DPE parallel to AB. Consider now an *element* of the screw propeller blade having an indefinitely small area (a) to be



placed at P, whose distance from the axis AB is denoted by (r). The *element* (a) thus placed at P has, therefore, a normal line, that is, there is a line perpendicular to it which is called a normal to the *element* (a). Let GF be the projection of this normal line upon the plane ABP; and, further, suppose the position of this normal through P to be determined by the angles α, β, γ, which it makes with AB, CP, and a line perpendicular to the plane ABP respectively. These angles are not, however, independent; they are connected by the well known equation

$$\text{Cos}^2\alpha + \cos^2\beta + \cos^2\gamma = 1 \quad \dots\dots\dots\dots\dots \quad 1$$

2. As CP revolves about the axis AB with an angular velocity (u), the *element* (a) of the screw propeller blade strikes the water in the direction of its normal line in accordance with the usual theory of fluids. And, therefore,

for any assumed angular velocity (u) the greater the velocity of the *element* is in its normal direction the greater is the force with which the *element* of the screw propeller blade will strike the water.

In consequence of this it is absolutely necessary to compute geometrically the normal velocity of the *element* (a) by means of the data supplied by the dimensions of the propeller, its angular velocity (u) and its velocity (v) in the direction of the axis AB, in order to determine the amount of resistance to angular motion and motion parallel to AB. This is the preliminary proposition the solution of which can be found without any reference to the theory of the resistance of fluids.

3. Suppose, then, that the *element* (a) placed at P is subject to the influences of two causes, viz. the velocity (ru) perpendicular to the plane ABP. and the velocity (v) parallel to the axis AB. The object now is to find an expression for the normal velocity arising from these two causes.

To do this it is only necessary to resolve (ru) the angular velocity and the velocity (v) parallel to AB into two directions, viz.: perpendicular and parallel to the normal line. Thus is obtained,

$rw\cos\nu =$ velocity parallel to normal.
$ru\sin\nu =$ velocity perpendicular to normal.

The latter velocity produces no effect upon the water, as friction in this case is entirely disregarded.

The former, however, is the velocity with which the *element* (a) of the propeller blade strikes the water in the case when there is no motion parallel to AB.

The effect of (v) the velocity parallel to AB, in the direction of the normal to the *element* (a) must be deducted from ($ru\cos\gamma$) in order to obtain the full normal velocity with which the *element* (a) strikes the water. The resolution of (v) in the direction of the normal will be $v\cos a$, hence, denoting the absolute normal velocity by N, the following equation obtains, viz.—

$$N = ru\cos\gamma - v\cos\alpha \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad 2$$

4. The normal velocity then as expressed by equation (2) can be made to assume different values by assuming different values of the available angles α, ν which determine the position of the normal line to the *element* (*a*) of the propeller blade.

The normal velocity of the *element* (*a*) will be zero, when the following relation between the angular velocity (*u*) and the velocity of translation (*v*) obtains, viz.

$$v. = \frac{ru\cos.\gamma}{\cos.\alpha} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad 3$$

This equation is derived directly from equation 2.

It appears, therefore, that when the *element* (*a*) moves in accordance with the relation (3) it produces no resistance whatever either to rotation or translation along the axis of the propeller. In this case the *element* (*a*) moves upon a *surface* which may be not inappropriately denominated the *surface* of *vanishing pressure.*

5. With reference to equation (1) Art. 1., it will be readily seen that the condition α, ν being complimentary (that is, $\alpha + \nu = \frac{\pi}{2}$) will give $\beta =$ zero. Hence, in this case, the normal line of the *element* (*a*) placed at P is situated in a plane at right angles to the radius (*r*). Further, if each of the two angles α, ν remains constant for the same value of (*r*) as well as satisfy the above condition, then the curve of the propeller at (*r*) distance from the axis will be the common helix of the Archimedean screw as used in Her Majesty's Navy.

6. In the determination of the formula 2 Art. 3 it was assumed that the *element* (*a*), with a normal velocity N, struck the water when perfectly at rest. The position, however, of the propeller practically placed at the stern of

the vessel which it propels, leads to the conclusion that this assumption is by no means a correct one. A slight correction in the formula 2 will therefore be necessary to meet the case when the element (*a*) strikes the water which has a velocity (V) in the direction of the axis A.B. By resolving V in the direction of the normal line the result will be $V\cos\alpha$. This resolved velocity must be added to N in equation (2), then it follows that the element (*a*) striking the water at rest with a normal velocity N^1 where

$$N^1 = ru\cos\gamma - (v - V)\cos\alpha \ldots\ldots\ldots\ldots\ldots (4)$$

will be exactly the same as the *element* (*a*) striking the water, having a velocity V in the direction of the axis, with a velocity $N = ru\cos\gamma - v\cos\alpha$.

In this case the surface of vanishing pressure is deduced from (4) by the relation

$$v = \frac{ru\cos\gamma}{\cos\alpha} + V \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad 5$$

Equation (5) will do good service in explaining the perplexing phenomenon of *negative slip* which has been one of the experiences of screw propulsion by means of the common Archimedean screw.

It will be observed that the *element* (*a*) of the screw propeller blade can be made to assume any position in space by assigning suitable values to the angles α, β, γ which fix absolutely the direction of the radius of curvature of the propeller blade at the given point P. Now the question naturally arises, from what has been said in the foregoing articles, viz., what values must be given to the angles α, β, γ at the point P, and how must they vary from point to point on the propeller blade, so that the resistance in the direction of the axis shall be a maximum when the resistance to angular motion is a given quantity ?

It is hardly necessary to add that the solution of the above problem is not easily accomplished. It is to be hoped, however, that practical experience in union with mechanics and geometry of the propeller blade will, ere long, give an answer sufficiently accurate for all practical purposes. The Council of the Society of Naval Architects have promised a Gold Medal to the best solution during the year 1879.

Dr. R. ANGUS SMITH, F.R.S., exhibited a section of a large Eucalyptus globulus, and drew attention to the peculiar arrangement of the fibres in the different rings of the wood.

Professor W. C. WILLIAMSON, F.R.S., pointed out that the peculiarities in the section of the Eucalyptus exhibited by Dr. Smith arose from differences between the inner and outer portions of each annual ring of the wood. In most plants the fibro-vascular structures of the inner portion and the sclerenchymatous wood-cells of the outer one had their axes parallel to each other as well as to the long axis of the stem. In the section of Eucalyptus exhibited this is not the case. The axes of the aggregated wood cells incline in one lateral direction and those of the vascular layer in an opposite one; hence when a section of the stem of the tree is split vertically these two tissues are seen to dovetail obliquely into each other in a very singular manner.

Professor WILLIAMSON also called attention to some recent discoveries throwing light upon the two rows of round or oval scars seen on the stems of the carboniferous genus Ulodendron. Mr. Carruthers long ago expressed his conviction that these scars marked positions from which ærial roots had been given off. On the other hand Professor W. had pointed out (Phil. Trans. 1872, pp. 210—225) the extreme probability that these scars had supported cones or strobili —that conclusion having been based upon the peculiar manner in which the vascular bundles supplying the scars were given off from the central vascular cylinder of the branch or stem. Dr. Dawson of Montreal had arrived at a similar conclusion from a study of the external forms of these scars. The question has been set at rest through the discovery, by Mr. D'Arcy Thompson of Edinburgh, of two specimens in which the cones are preserved in connection with the scars. Each cone was obviously developed from a leaf-covered bark, but as it grew in diameter its truncated base pressed down the neighbouring leaves and thus produced the well-known large circular areola. The actual organic connection of the cone and its parent branch was obviously limited to a small circular areola in the centre of each scar. After these deciduous strobili fell, the scars remained permanently impressed upon the bark of the tree. At first each scar was small and circular, but as the stem enlarged both in length and diameter the scars underwent a similar enlargement; but since the longitudinal growth of the stem was obviously more rapid than the transverse one, it necessarily resulted that these originally circular scars became converted into vertically elongated oval ones. This change in the shape of leaf-scars resulting from the direction of growth is well shown in the arborescent stems of living species of Alsophila. Ulodendron therefore was a branching Lepidodendroid plant whose branches supported distichously arranged strobili, the individuals of the two series being disposed alternately.

PHYSICAL AND MATHEMATICAL SECTION.

March 25th, 1879.

E. W. BINNEY, F.R.S., F.G.S., in the chair.

The following gentlemen were elected officers of the section for the ensuing year:—

President.
E. W. BINNEY, F.R.S., F.G.S.

Vice-Presidents.
JOSEPH BAXENDELL, F.R.A.S.
ALFRED BROTHERS, F.R.A.S.

Treasurer.
JAMES BOTTOMLEY, D.Sc., B.A., F.C.S.

Secretary.
REV. THOMAS MACKERETH, F.R.A.S., F.M.S.

"Observations of Dr. Klein's new Lunar Crater near Hyginus, made at the Observatory, Birkdale," by JOSEPH BAXENDELL, F.R.A.S.

In consequence of the conflicting statements which had been made with reference to the object in the moon near Hyginus discovered by Dr. Klein on the 19th of May, 1877, which he believed to be a new crater; and some observers having doubted its existence, I took advantage of the fine night of the 2nd of November, 1878, to turn the telescope on the moon, which was then in a phase favourable for the observation of the region of Hyginus, and having Dr. Klein's drawing to refer to, I at once saw the object which has been made the subject of so much discussion and dispute. It appeared at 6h. 15m. G.M.T. as a round and very dark spot, and was slightly less black than the shadow in Triesnecker. With powers 180 and 260 its diameter was estimated to be about two-thirds that of the crater marked by Dr. Klein Triesnecker *d* (Beer and Mædler's Hyginus *a*); and in moments of best vision I received the impression of an extremely delicate line of light on its eastern border. The tongue-shaped depression towards the south mentioned by Dr. Klein was also seen, and the two minute craters to the south-west laid down in his sketch were very conspicuous.

At 6h. 45m. the blackness of the new crater had already become very much less than that of the shadow in Triesnecker.

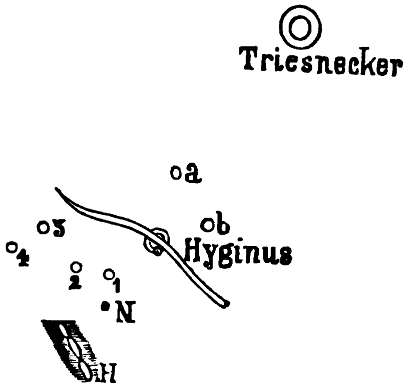
November 3rd, at 5h. 15m. The new crater still appeared as a dark spot, though not nearly so dark as on the previous evening.

November 4th. The new crater appeared as an irregular ill-defined spot, slightly brighter than the surrounding district.

December 4th, at 8h. Klein's crater presented the same appearance as on November 4th.

December 5th, at 14h. 15m. I noted down "Klein's new crater is now a moderately bright spot."

Owing to illness I was unable to observe the moon again near its first quarter till February 28th, 1879, when at 9h. 30m. the new crater was again seen as a very dark though not absolutely black spot; but at 11h. 30m. it had become decidedly less dark. A line drawn from the middle of Triesnecker through the middle of Hyginus and carried about one-fourth of its length further will fall upon the new crater, the position of which is also well-marked by a line drawn from the small crater No. 3 in annexed sketch, through No. 2, and continued as far again.



March 1st, 7½h. Klein's crater well seen with 4in. aperture. With 6in. aperture it appears smaller and much less dark than last night. At 10h. the crater is sensibly less dark since 7½h., and has now an irregular oval shape in the direction of the small crater No. 1.

March 2nd, 6h. 35m. The crater no longer appears either as a round or oval shaped spot, but on its place and extending nearly to the first or No. 1 small crater, to the S.W. there is a dusky marking but little less bright than the surrounding district. 7h. 15m. Definition has now become so bad that it is impossible to make out any of the details satisfactorily, or even to distinguish the dusky marking.

March 3rd, 9h. 30m. Klein's crater is not now visible as either a dark or a bright spot.

March 4th, 9h. There is a moderately bright spot on or very near the place of the crater; it is less bright than either of the two small craters to the S.W. Images unsteady, and observation interrupted by clouds.

March 5th, 12h. There is a faintly bright spot on the place of the new crater. The order of brightness of Nos. 2, 3, 4 and N (the new crater) is 4, 3, 2, N. N appears to be elongated, or rudely shaped like the figure 8, in the direction of crater No. 1.

March 6th, 7h. Klein's crater is still visible, slightly brighter than the surrounding ground. Order of brightness —4, 3, 1, 2, N.

March 8th, 12h. Klein's crater and the other small craters very distinctly visible, definition being moderately good. Order of brightness—4 = 3, 2, 1, N. A moderately bright streak from H to N with a faint continuation to a point on the furrow a little to the east of Hyginus.

I have had no opportunity of observing the moon since the 8th of March, but the observations up to that date, as given above, seem to me to remove all doubt as to the existence of the object which Dr. Klein regards as a new crater. On the supposition that it is not of recent formation, it seems unaccountable how it could have entirely escaped the notice of such experienced observers as Schrœter, Lohrman, Beer and Mædler, Schmidt, Nasmyth, Birt, Neison, Klein, and others who have devoted so much of their time and attention to the critical examination and mapping of the moon's surface; and I think, therefore, we are entitled, in the absence of positive evidence to the contrary, to conclude with Dr. Klein that it is a new appearance, though it may be doubted whether it is a true crater, as supposed by its discoverer, or merely a slight subsidence of a portion of the moon's surface.

"On the Meteorological Effects of the position of the Moon with respect to the Sun," by JOSEPH BAXENDELL, F.R.A.S.

In a paper "On the Variations of the Daily Range of Atmospheric Temperature as recorded at the Kew Observatory;" by Professor Balfour Stewart, F.R.S., printed in the Proceedings of the Royal Society, vol. xxv, p. 577, the author has shown that the daily range of temperature varies with the phases of the moon, and that the variations are greater in the winter than in the summer months, and opposite in character, the range in the winter being greatest at and shortly after new moon, and least at and a few days after the full. The novel and remarkable character of this result of the Kew observations at once suggested the desirability of discussing the observations made at other stations with reference to the phases of the moon, though it is evident that as the daily range of temperature is dependent to a considerable extent on the greater or less prevalence of cloud, the direction of the wind, the hygrometric state of the air, &c., its variations could not in general be the same at all stations in the same phase of the moon, but that while at a maximum at certain stations during a given phase it would be at a minimum at other stations. Moreover, it seemed very probable that if the daily range of temperature is sensibly influenced by the position of the moon with respect to the sun, other elements, as the amount of cloud, the mean daily temperature, direction of the wind, &c., would also be found to be subject to changes in a period corresponding to that of the moon's synodical revolution. I therefore in the first instance selected for examination the observations which have been regularly and carefully made at the Southport meteorological observatory since the 1st of July, 1871. From the open position of the observatory, removed many miles from hills or high land, the observations are less likely to be affected by disturbing influences than at stations less favourably situated,

The following table shows the mean daily range of temperature on the days of full moon, and two days before, and two days after, during the winters of the years 1871-8, and also the mean daily range on the days of new moon, and two days before, and two days after, during the same winters:—

Winters of		Mean daily Range of Temperature on days of Full Moon.	Mean daily Range of Temperature on days of New Moon.	Difference. Full—New.
1871-2	...	9·09	10·28	−1·19
1872-3	...	9·41	8·56	+0·86
1873-4	...	8·05	9·21	−1·16
1874-5	...	10·35	9·96	+0·39
1875-6	...	9·37	8·39	+0·98
1876-7	...	11·58	9·08	+2·50
1877-8	...	9·66	9·33	+0·33
Means	...	9·64	9·26	+0·38

It appears, therefore, that at Southport during the winter months the daily range of temperature is on the average greater on the days of full moon than on the days of new moon, a result the reverse of that derived by Dr. Stewart from the Kew observations, and confirming the view stated above that the maximum range cannot take place at all stations in the same phase of the moon.

Estimations of the amount of cloud are made and recorded at 9 a.m., 1 p.m., and 9 p.m. daily. I have therefore tabulated the amounts of cloud at the times of full and new moon during the six winter months on the plan adopted with the daily range of temperature, and have obtained the following mean results :—

		Mean Amount of Clouds at 9 a.m. on days of	
		Full Moon.	New Moon.
1871-2	...	7·7	7·1
1872-3	...	6·3	7·4
1873-4	...	7·2	7·5
1874-5	...	6·9	7·6
1875-6	...	7·9	7·5
1876-7	...	7·7	8·4
1877-8	...	8·0	7·9
Means	...	7·38	7·63

		Mean Amount of Cloud at 1 p.m. on days of	
		Full Moon.	New Moon.
1871-2	...	7·2	6·0
1872-3	...	6·5	6·3
1873-4	...	6·9	7·9
1874-5	...	6·6	7·0
1875-6	...	7·7	7·2
1876-7	...	7·9	8·3
1877-8	...	7·5	7·3
Means	...	7·19	7·14

		Mean Amount of Cloud at 9 p.m. on days of	
		Full Moon.	New Moon.
1871-2	...	6·3	5·9
1872-3	...	6·2	5·9
1873-4	...	6·2	5·4
1874-5	...	7·1	4·8
1875-6	...	6·6	5·7
1876-7	...	8·0	6 6
1877-8	...	7·5	7·1
Means	...	6·84	5·91

It appears, therefore, that at 9 a.m. the mean amount of cloud is slightly greater on days of new moon than on days of full moon; at 1 p.m. the amounts are sensibly equal, but at 9 p.m. the amount on days of full moon is greater than on days of new moon. It will also be seen that while on days of full moon the mean diminution in the amount of cloud between 9 a.m. and 9 p.m. is 0·54; on days of new moon it amounts to 1·72.

It has been supposed by Sir John Herschel and other meteorologists that the full moon had the effect of partially dispersing the clouds, but the above results show very clearly that any effect which the moon may have is of an opposite character, as in every winter the mean amount of cloud was greater at the time of full than of new moon.

The observations of mean daily temperature at the times of full and new moon have yielded the following results :—

		Mean Daily Temperature on days of		
		Full Moon.	New Moon.	
		°	°	
1871-2	...	44·3	43·4	+0·9
1872-3	...	40·3	43·0	−2·7
1843-4	...	45·4	46·0	−0·6
1874-5	...	41·6	41·9	−0·3
1875-6	...	38·2	43·5	−5·3
1876-7	...	43·7	45·4	−1·7
1877-8	...	45·9	44·5	+1·4
Means	...	42·77	43·96	−1·19

The days of the full moon were therefore on the average 1·19 colder than the days of new moon. This result led me to infer that at the times of full moon northerly winds had been more frequent than at the times of new moon. Observations of the direction of the wind are made at 9 a.m., 1 p.m., and 9 p.m. daily; and as the velocity of the wind is generally greatest about 1 p.m., its general direction can be more accurately determined at that time, and therefore a table was formed, showing its direction at 1 p.m. on the selected days at the times of full and new moon during the winter months of the seven years, and the following results were obtained :—

From the	Wind Frequency at the Times of Full Moon.	Wind Frequency at the Times of New Moon.	Difference. Full—New.
N.E quarter.	35 days.	18 days.	+ 17
S.E. do.	61 do.	84 do.	- 23
S.W. do.	62 do.	82 do.	- 20
N.W. do.	52 do.	26 do.	+ 26

At the times of full moon the winds were from the N.E. and N.W. quarters on 87 days, but at the times of new moon on only 44 days, thus confirming the conclusion to which I had been led by the results of the temperature observations.

A similar grouping of the direction of the wind at 1 p.m. on the days of first quarter of the moon and two days before, and two days after; and also on the days of last quarter, and two days before and two after, gave the following results :—

	Wind Frequency at Times of First Quarter.	Wind Frequency at Times of Last Quarter.	Difference. Last Qr.—First Qr.
N.E. quarter.	18 days.	25 days.	+ 7
S.E. do.	70 do.	70 do.	0
S.W. do.	74 do.	66 do.	- 8
N.W. do.	48 do.	46 do.	- 2
Calm ...	0	3	+ 3

From these two tables of wind frequency we find that the relative frequency of northerly and southerly winds at 1 p.m. during the winter months under the different phases of the moon is as follows :—

	Frequency of Northerly Winds.	Frequency of Southerly Winds.	Ratio of Southerly to Northerly Winds.
New Moon	44 days.	166 days.	3·77
First Quarter	66 do.	144 do.	2·18
Full Moon	87 do.	123 do.	1·41
Last Quarter	71 do.	136 do.	1·91

These numbers show that northerly winds are least frequent at and a little after new moon, and most frequent at and a little after full moon. On the other hand southerly winds are least frequent under the full moon, and most frequent under the new moon.

Taking now the observations of the direction of the wind made at 9 p.m. we obtain the following results :—

	Frequency of		Ratio of Southerly to Northerly Winds.
	Northerly Winds at 9 p.m.	Southerly Winds at 9 p.m.	
New Moon	43 days.	162 days.	3·76
First Quarter	65 do.	138 do.	2·12
Full Moon	89 do.	116 do.	1·30
Last Quarter	75 do.	128 do.	1·70

The differences between the numbers in the last column of each of these two tables are so great that it seems impossible to doubt that the position of the moon with respect to the sun exercises in some way an influence upon the direction of the wind, and that this is the case is further supported by the fact that in each winter the frequency of northerly winds under the full moon and last quarter was greater than under the new moon and first quarter, as will be seen from the following numbers :—

		Northerly Winds at 1 p.m. and 9 p.m. under	
		New Moon and First Quarter.	Full Moon and Last Quarter.
1871-2	...	21	49
1872-3	...	36	56
1873-4	...	30	37
1874-5	...	34	52
1875-6	...	40	57
1876-7	...	26	39
1877-8	...	31	32

These numbers also indicate that the frequency of northerly winds under the full moon and last quarter, as compared with that under the new moon and first quarter, has gradually diminished during the seven years. Thus taking groups of three winters each, we have the following mean ratios :—

1871-2 to 1873-4 the mean ratio is ... 1·63
1873-4 to 1875-6 do. do. ... 1·40
1875-6 to 1877-8 do. do. ... 1-32

Assuming that this diminution is due to the gradual diminution of solar activity which has been going on during the seven years we may expect that the ratio will be at a minimum during the present winter.

The widely different character of the action of the moon upon the direction of the wind during the summer months of the seven years will be seen from the following results:—

	Frequency of Northerly Winds. 1 p.m.	Frequency of Southerly Winds. 1 p.m.	Ratio of Southerly to Northerly Winds.
New Moon	89	123	1·38
Full Moon	84	131	1·56

The ratio of southerly to northerly winds is therefore slightly less under the new than under the full moon in the summer months, but in the winter months, as we have already seen, it is much greater.

OXFORD OBSERVATIONS.

As the influence of the moon appeared from the Southport observations to be shown more clearly by the direction of the wind than by the daily range of temperature, the amount of cloud, or the mean daily temperature, the observations of the wind made at the Radcliffe Observatory, Oxford, during the ten years 1865-75 were tabulated on the plan I had adopted with the Southport observations, though it must be remarked that the directions of the wind given in the published "Observations" are not for any particular hour of the day or night, but are the estimated mean directions for the 24 hours. It was considered, however, that this difference would have no material effect upon the final results. The following table shows the frequency of northerly winds under full moon and new moon during the ten winters 1865-6 to 1874-5:—

	Frequency of Northerly Winds under Full Moon.	Frequency of Northerly Winds under New Moon.
1865-6	12	6
1866-7	14	12
1867-8	14	7
1868-9	17	14
1869-70	21	13
1870-1	11	14
1871-2	13	5
1872-3	19	5
1873-4	8	6
1874-5	15	8
Totals	144	90

It will be seen that in every year except one the frequency under the full moon was greater than that under the new moon, and that the total frequency of the ten winters was 144 under the full moon and only 90 under the new moon. These results of the Oxford observations are therefore in very satisfactory accordance with those of the Southport observations, and give strong support to the view that the direction of the wind is in some way influenced by the position of the moon with respect to the sun. Should this view be confirmed by the results of observations made at other stations it will evidently be very desirable to examine the directions in which the centres of cyclones and anticyclones move across our coasts, during the winter months, under different phases of the moon.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 10th, 1879.

Thomas Alcock, M.D. in the chair.

Mr. Charles William Kimmins, Mr. Sydney Young, and Mr. Marcus Manuel Hartog, B.Sc., F.L.S., all of Owens College, were elected Associates of the Section.

Mr. **Charles Bailey**, F.L.S., gave an account of the germination of ferns, with diagrams.

Mr. M. M. **Hartog**, F.L.S., described the anatomy of the fresh water mussel (Anodonta Cygnea (L).)

March 10th, 1879.

A. **Brothers**, F.R.A.S., in the chair.

Mr. S. **Moore** exhibited a curious white growth, probably attributable to spores of a species of Polyporus, covering beech trees in Derbyshire.

Mr. **Rogers** exhibited rings from the scales of cuttle fish (Loligo), brought up abundantly by two tides only at Barmouth, in 1874, collected by Mr. R. M. Christy.

Mr. **John Plant**, F.G.S., read a paper upon "The Great Sheatfish, *Silurus glanis*, in Loch Bad-a-Luacradh, Ross." The author gave a sketch of the natural history of the family of the Siluridæ, which includes about a dozen known species, one of which the S. glanis inhabits some of the great rivers of Europe and a few of the lakes. It is most abundant in the Danube, Volga, and the Rhine, and is known in the largest streams which fall into the Baltic, being also at times caught in the upper regions of the Baltic, where the water is but slightly brackish. It has been obtained from Lakes

Neuchatel, Brienne, and Morat, the species is also found in North American lakes and some rivers.

The Sheatfish grows to an enormous size in waters favourable to its mode of life. Specimens weighing 700 lbs. are recorded from the Danube, and in America, the average size of the adult fish is about 300 lbs. The bulk of the fishes caught in the season are of less weight, The length of exceptional specimens will reach to 20 or 22 feet; but 8 or 10 feet are the lengths of large specimens.

The general appearance of the Sheatfish is like that of a bulky eel, the eyes are large and froglike. The head is unpleasant to look at, and the mouth wide—the upper lip is armed with two long wormlike feelers, or barbules, which are kept in active motion either as sensitive organs of feeling, or to seize frogs and small fish which come within their reach. Its habits are to hide in the muddy bottoms or amongst the roots of aquatic vegetation—only coming to the surface on hot sunny days, after thunderstorms or when the water is frozen over—to keep an air hole open for occasional fresh air breathing.

It has been attempted twice to introduce its spawn or young fish in English rivers, both trials have failed, although it is a hardy fish and very tenacious of life. The flesh of the Sheatfish is largely eaten in the countries where it abounds, but accounts differ as to its flavour and qualities. At one time in season it resembles fine fresh salmon in flavour, at another time white, fat, soft, luscious and not easy to digest. In the restaurants in the towns along the Danube, the Sheatfish is cooked in so many ways that a traveller may dine altogether upon this fish, and fancy he has been served with a variety of soups and meats.

Whether the Silurus was ever, or is now, native to British rivers and lakes is as yet an open question, the peculiar spine which supports the pectoral fin has been dug from deposits in the London clay, *i.e.*, *Eocene*, and Mr.

Higgins, of Liverpool, found one of these spines in clay under a bed of peat at Leasowes, on the banks of the river Dee. So far it may have lived in English rivers at more remote times.

In 1828 a fish was caught in a river at Florence Court, Ireland, which was satisfactorily proved a long time afterwards by the Earl Enniskillen and Professor Louis Agassiz to be a *Silurus glanis*—not a fragment of the fish or its skeleton was preserved, which was unfortunate, as its identification depended upon memory alone and its resemblence to a drawing of Silurus.

Dr. Fleming notices a remark by Sibbald, that the Silurus may have been seen in the Scotch rivers in his day.

The author then stated: " Several years ago I received a letter from a gentleman residing in the highlands of Ross describing an extraordinary monster which had been occasionally seen by his servants and tenants floating on the waters of Loch Bad-a-Luacradh—Lake of the Rushes—near the coast about Loch Eu. The people called the monster a Snail Whale; it seemed about 22 feet in length, and had two flexible horns on its mouth; it was fond of basking on the surface of the water, particularly after great storms, and looked very much like a herring boat turned keel upwards. In reply I sent a drawing and description of *Silurus glanis,* which was at once recognised by all the people who had seen the big monster as a capital portrait of it.

Efforts were repeatedly taken to capture the monster by nets, by baiting, and by shooting, but without success, and for three winters similar endeavours to capture it equally failed. It was ascertained by strict inquiry amongst the native residents, that the fish had been seen as far back as sixty years ago, when it was much smaller. An old shepherd had seen it first, a very old dame saw it thirty years ago, a smuggler or illicit whiskey distiller who had a bothy hard by the loch often saw the monster in the quiet hours of morn, he did not like the uncanny beast as a neighbour at all. Many other witnesses who gave similar evidence were mentioned by the author, who as well described the methods adopted to capture the monster, but after much trouble and expense it proved too wary and alert to be captured or even to be shot, and the enterprise had to be abandoned. Probably the monster died, or escaped from the loch.

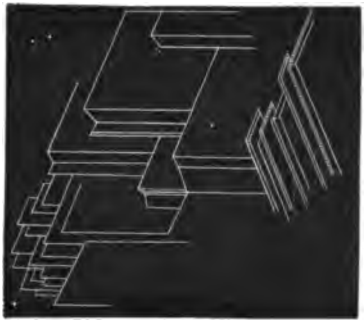
Ordinary Meeting, April 15th, 1879.

J. P. JOULE, D.C.L., LL.D, F.R.S., &c., President, in the Chair.

"Mineralogical Notes," by **CHARLES A. BURGHARDT**, Ph.D., the Owens College.

Precious Garnet (Almandine) from Ramsbottom, Lancashire.

A few days ago I received a small specimen of rock from a former pupil of mine (Mr. J. F. C. Sieber) accompanied by a note, in which the writer gave particulars respecting the locality in which the rock was found, and asked me to examine certain red granules present in the rock in order to ascertain whether they were garnets or not. The rock itself is a conglomerate of milky quartz grains (varying considerably in size), cemented together with silica and calcium carbonate; and disseminated throughout this conglomerate are the garnet grains in question, the whole constituting a 14 feet "fault" in the coal seam known as the Sand-rock or Featheredge Seam in the "Shipperbottom Mine," near Grant's Tower at Ramsbottom. The coal from this mine is accompanied by a shale, which contains a very considerable amount of sulphate of aluminium, as it effloresces on being exposed to the action of the air; becoming eventually perfectly white like a piece of chalk. A microscopical examination of the rock showed the garnets to be very irregular in form and to vary considerably in size, the largest attaining a width of about 2·25 mm. and the smallest about 0·75 mm. They exhibited no distinct crystalline form, but a peculiar indented appearance was observed on the surfaces of some individuals, whilst others were pebblelike, somewhat convex and rounded off as though they had been mechanically acted upon by water. The quartz grains did not exhibit the slightest trace of crystalline form but were rounded off and pebble-like. Both the garnets and the quartz grains could be extracted from the matrix, a cast of their forms being left behind. The red grains fused easily and quietly before the blowpipe to a black bead, which was not however magnetic; they were somewhat attacked by hydrochloric acid with a slight separation of powdery silica: the presence of iron was also detected. The hardness of the mineral was 7·5, and its specific gravity 4·09 at 9°C. I hope shortly to determine the chemical composition. A microscopical examination of the garnet grains showed them to be crystalline, exhibiting certain marked peculiarities, and after a careful examination of many of them I came to the conclusion that the form in which they crystallised was the rhombicdodecahedron. I had previously proved them to be crystals of the regular system by examining the grains in polarised light and finding them to be isotropic. The crystals were built up out of myriads of minute laminated abnormal rhombicdodecahedrons, two faces of which have been enormously developed at the expense of the other faces. These laminæ are arranged parallel with each other in positions corresponding to each face of a rhombicdodecahedron, the indentations observed upon the indistinct crystal faces of some individuals being caused by this peculiar laminated growth. The laminæ vary from 0·0282 mm. to 0·1880 mm. in length, from 0·0188 mm. to 0·1504 mm. in width, and from 0·0031 mm. to 0·0045 mm. in thickness. I have endeavoured to give some idea of the appearance exhibited by these laminæ when examined under the microscope in the drawing appended.



GARNET LAMINÆ FROM RAMSBOTTOM.

The colour of the garnets (for such the crystals undoubtedly are) is light rose-red, and coupling this with their transparency and chemical properties there can be no doubt that they are alumina-iron garnets, or *almandine*. On examining some of the laminæ under the microscope I observed that they contained enclosures, which were mostly fluid cavities or "gaspores," also here and there cracks and rifts, filled with a white substance which most probably was calcite. On some of the laminæ there was a separation out of a greenish mossy substance, which I believe to be ferric oxide, and on one individual I observed a few specks of iron-pyrites. It is somewhat difficult to draw a conclusion as to the origin of these garnets in such a locality. From the macroscopical examination of the rock it would appear that the cement and the garnets were *both* in a soft and pasty condition, the latter being probably formed whilst in the pasty matrix and prevented from attaining a normal development. It is a well known fact that crystals which have formed in a free space or in a liquid mass which has not exerted any retarding influence upon the force of crystallisation, always exhibit well defined faces and angles, whilst the opposite conditions result in abnormal growths such as the garnet crystals just referred to. The quartz grains were probably formed previously to the garnets, as they appear to be waterworn pebbles, and were most likely carried along with the semi-paste-like mass which contained the constituents of garnet and calcite in its substance. When the paste hardened, the quartz grains and the garnets were enclosed in the manner exhibited on the specimen of rock from Ramsbottom. All the constituents of garnet were at hand, namely, aluminous shale, which would furnish the alumina and silica, and iron-pyrites, which would furnish the iron, whilst water impregnated with calcium carbonate, acting upon the shale and iron-pyrites, would bring about the necessary chemical changes, calcium sulphate being formed and carried away and the garnet solution (if such a term may be used) could then crystallise out. From the formation and the locality in which the garnets were found it is quite certain that their origin was aqueous. Another explanation of the occurrence of garnet in the conglomerate may be as follows, viz.: garnet is a very common accessory of crystalline rocks, occurring mostly in slates of various kinds, such as talc-slate, mica-slate, chloritic-slate, and aluminous-slate; it also occurs in gneiss, granite, porphyry, serpentine, and granular limestone. One or more of the above-mentioned rocks containing crystallised garnets already formed, were disintegrated by the action of water, and the resulting debris (consisting of grains or pebbles of quartz and garnets) was carried away and deposited in a semi-fluid mass of silica and calcite and thus cemented together. The question which next arises is, where and what is the original garnet rock in the neighbourhood of Ramsbottom; is it perhaps the aluminous shale? This problem still remains to be worked out. It is evident however at once on examining the conglomerate that there is a large excess of quartz grains present, with a very fair sprinkling of garnets; whilst the cement is not present in any great amount. An examination of thin sections of this rock under the microscope may clear up some doubts, and throw further light upon its origin.

Fibrous Rock-Salt (Sodium Chloride).

This anomalous formation has always been an interesting one, on account of the strong cubical "habit" of rock-salt, all other forms than that of a well-defined cube being rarities. The fibrous formation is not due to a crystallisation of the sodium chloride in another system than the regular system, but is explained by an abnormal development of four cubical faces parallel to one axis, which may with propriety be called the prismatic axis. So far as I am aware, the formation of fibrous rock-salt has not been explained. Having the well known fact before me, viz., "the influence exerted upon the crystal form of a body crystallising out of its solution by the presence in that solution of a small quantity of a foreign body," I thought it probable that a similar effect might be produced upon the growth of sodium chloride crystals by the presence of a foreign body or bodies. In order to ascertain this I made a strong saturated solution of ordinary table-salt, filtered it, and precipitated out most of the salt by passing hydrochloric acid gas into the solution. The liquid was drained off from the precipitated salt and allowed to evaporate *very slowly* for several days in a narrow-necked flask and then laid aside to cool. I then observed amongst the mass of ordinary salt cubes numerous long, well-developed prisms. Some of these prisms were an inch or more in length and sometimes terminated by a broad cube; others again were twinned according to the usual law, viz., "the twin plane a face of the octohedron." I observed that many of the cubical crystals were somewhat bent and resembled closely the bent barytes crystals of the prism and basal terminal plane in combination. Nearly all the crystals (prismatic and cubical) were opaque or almost so, and of a very white colour. In addition to free hydrochloric acid, I found in the solution a quantity of magnesium sulphate and magnesium chloride, and it is to the peculiar retarding action of these substances, either together or singly, that I ascribe the general formation of fibrous rock-salt; for they could easily occur *natu-*

rally in any rock-salt formation. As sulphate of magnesium and chloride of magnesium are constituents of sea-water and generally present in all waters flowing from rocks, either crystalline or sedimentary, their presence during the deposition of rock-salt is ensured, whilst the presence of hydrochloric acid would also be possible if a slight rise of temperature took place during the deposition, it being a fact that magnesium chloride gives off hydrochloric acid during evaporation at comparatively low temperatures.

In the discussion which followed the reading of the above paper Mr. BINNEY, F.R.S., read an extract from Trans. Man. Geol. Soc., Vol. I., p. 87, to the following effect, viz.—

"On the Marine Shells found in the Lancashire Coal Fields, by E. W. Binney.

"Immediately above the 'Rough rock' at Birtle Dean, near Heywood, is the Featheredge coal, about two feet thick; this seam has a roof of one yard in thickness, consisting of black shale which is full of the Pecten, Goniatites, Povidonia, &c., mixed with various species of Ferns, Lepidodendra, and Sigillariæ. It is a singular fact that the black shale has scarcely ever been seen on the roof of this coal, except at Messrs. Ramsbottom's colliery, at Birtle Dean. At Fecit, within two miles of the last named place, and other localities, the roof of the coal consists of a coarse conglomerate of quartz pebbles, containing numerous garnets and crystals of carbonate of barytes, generally presenting a water-worn appearance. I have also observed small pieces of opal imbedded in this rock."

E. W. **Binney**, F.R.S., V.P., exhibited a gigantic tooth of a fossil shark found in making the railway between Seville and Cordova in Spain. The age of the deposit in which it was found is not clearly ascertained, but it is probably Older Pleiocene. It is the largest that he ever heard of. Neither the very large specimen in the Hunterian Museum, London, from Malta, nor any in the numerous public museums or private collections in England, equal that shown. Its dimensions are 6½ inches in perpendicur height and 4½ inches in breadth, and 10 inches in girth at its base. It is in a most beautiful state of preservation, nearly as perfect as it was in the living fish, and probably belonged to *Carcharias vulgaris* or some allied species.

Annual General Meeting, April 29th, 1879.

J. P. JOULE, D.C.L., LL.D, F.R.S., &c., President, in the Chair.

Report of the Council, April, 1879.

The annual statement of the Treasurer appended to this report shows that the expenditure of the Society during the year ending 31st March last, has considerably exceeded the income, owing principally to an unusually heavy outlay for books and book-binding. The balance against the General Fund Account has increased from £10 14s. 4d. to £113 0s. 11d. There is also a balance of £3 9s. 1d. against the Natural History Fund Account; and deducting these two sums from the Compounders' Fund, £125, there is a balance of £8 10s. in favour of the Society on the 1st of April, 1879.

The number of ordinary members on the roll of the Society on the 1st of April, 1878, was 157, and eight new members have since been elected; the losses are—deaths 2; resignations 2; defaulter 1. The number on the roll on the 1st instant was 160. The deceased members are Mr. Thomas Livesey aud Mr. James Mc.Connell. Since the above-mentioned date notice has been received of the death of Mr. Robert Stuart, the oldest member of the Society, he having been elected on January 21st, 1814.

It was stated in the last annual report that the Natural History Fund of £1500, presented by the late Natural History Society, had been received from the Trustees of the Owens College by the Treasurer, and that the Council had given instructions for its investment in any of the stocks or securities authorised by the trust deed. In accordance with these instructions the Fund has since been invested in Great Western Railway 5 per cent Preference Stock.

Since the last annual meeting three valuable portraits have been presented to the Society—the first a portrait of Dr. Percival, the first President of the Society, painted by Crozier, and presented by the Hon. Librarian, Mr. Nicholson; the second a portrait of the late Professor Eaton Hodgkinson, painted by Duval, and presented by Sir Thomas Fairbairn, Bart.; and the third a portrait of Mr. Binney, F.R.S., painted by Mr. William Herbert Johnston, and presented by the President, Dr. Joule, F.R.S. A valuable engraving of the late Mr. J. C. Dyer has also been presented by Mr. Nicholson.

In consequence of an application from the Geological Society for increased accommodation for their Library, the following arrangement was agreed to by the Council on the 7th of January last:—"That the Geological Society should have shelf space given them in the Entrance Hall on both sides; that they should remove the inner door, moving the outer door at their expense, and make good all damages done by the alterations; that they should have storing in the cellar for their Proceedings; that the members of the Geological Society shall have the use of the Council Room at all times when it was not occupied by the Council or by Committees, and that they should make an annual donation to the housekeeper for sending out their circulars."

At a meeting held on the 15th of October, 1878, the Council resolved "That Associates of Sections be empowered to vote for the election of officers of the Sections, such officers being members of the Society."

As the centenary of the Society will occur in February, 1881, it will be for the new Council to consider and decide upon the steps which it may be thought desirable to take for its formal celebration.

The following papers and communications have been read at the Ordinary and Sectional Meetings of the Society during the session:—

October 15*th*, 1878—"On a Eucalyptus globulous," by E. W. Binney, F.R.S., F.G.S.

"Relative Brightness of the Planets Venus and Mercury," by James Nasmyth, C.E., F.R.A.S., Corresponding Member of the Society.

"On the Water of Thirlmere," by Harry Grimshaw, F.C.S., and Clifford Grimshaw.

October 29*th*, 1878.—"Note on certain Thionates," by E. J. Bevan, Student in the Owens College. Communicated by Professor H. E. Roscoe, F.R.S., &c.

"The Use of the Opercula in the Determination of the Cheilostomatous Bryozoa," by Arthur Wm. Waters, F.G.S.

"On some Improved Methods of Producing and Regulating Electric Light," by Henry Wilde, Esq.

November 4*th*, 1878.—"An Account of three visits to the Breidden Hills, Montgomeryshire, North Wales, in May, June, and July, 1877," by J. Cosmo Melvill, F.L.S.

November 12*th*, 1878.—"Remarks on a Fossil Plant found at Laxey, in the Isle of Man," by E. W. Binney, F.R.S., F.G.S.

November 26*th*, 1878.—"On some Improved Methods of Producing and Regulating Electric Light," Part II, by Henry Wilde, Esq.

December 2*nd*, 1878.— "A Preliminary Abstract of an Investigation of the Nervous System of Cyclops," by Marcus M. Hartog, F.L.S.

December 10*th*, 1878.—"On the Combinations of Aurin with Mineral Acids," by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

"On the Estimation of Small Excesses of Weight by the Balance from the Time of Vibration and the Angular Deflection of the Beam," by J. H. Poynting, B.A., B.Sc.

December 24*th*, 1878.—"Note on the Intensity of Moonlight," by Harry Grimshaw, F.C.S.

January 7*th*, 1879.—"On Boulders of Clay from the Drift," by E. W. Binney, F.R.S., F.G.S.

January 8*th*, 1879.—"On Specimens of Ballast Plants collected at Cardiff in September, 1878," by Mr. Thomas Rogers.

January 21*st*, 1879.—"On an old Letter of the late Sir Walter Scott," by E. W. Binney, V.P., F.R.S., F.G.S.

"On a further Analysis of the Water of the Mineral Spring at Humphrey Head," by C. Grimshaw and H. Grimshaw, F.C.S.

January 28*th*, 1879.—"On a Copy of the Principia of Newton," by James Bottomley, B.A., D.Sc.

February 4*th*, 1879.—"The Area of the Middle Drifts, as determined by their Contents," by Alfred Bell, F.G.S. Communicated by R. D. Darbishire, F.G.S.

February 18*th*, 1879.—"On a Chemical Investigation of Japanese Lacquer, or Urushi," by Sadamu Ishimatsu. Communicated by Professor Roscoe, F.R.S.

"On the Bursting of the Gun on board the Thunderer," by Professor Osborne Reynolds, F.R.S.

February 25*th*, 1879.—"On the Mean Temperatures of the Winters of the last 29 years," by the Rev. Thomas Mackereth, F.R.A.S. &c.

March 4*th*, 1879.—"On a Modification of Bunsen's Calorimeter," by Professor Balfour Stewart, LL.D., F.R.S.

"The Poisonous Qualities of the Yew," by William E. A. Axon, M.R.S.L., F.S.S.

March 10*th*, 1879.—On the great Sheatfish, *Silurus glanis*, in Loch Bad-a-Luacradh, Ross," by John Plant, F.G.S.

March 18*th*, 1879.—On Siliceous Fossilization," Part II., by J. B. Hannay, F.R.S.E., F.C.S.

March 25*th*, 1879.—"Observations of Dr. Klein's new Lunar Crater near Hyginus, made at the Observatory, Birkdale," by Joseph Baxendell, F.R.A.S.

"On the Meteorological Effects of the Position of the Moon with respect to the Sun," by Joseph Baxendell, F.R.A.S.

April 1*st*, 1879.—Two Deductions from Mr. G. H. Darwin's letter in *Nature*, February 6th, 1879, upon Sir W. Thomson's equation of cooling

$$v = v_0 + \frac{2V}{\sqrt{\pi}} \int_0^{\frac{x}{2\sqrt{kt}}} \varepsilon^{-z^2} dz ,"$$

by Mr. James Heelis.

"Arrangement of a Tangent Galvanometer for lecture-room purposes, to illustrate the laws of the action of currents on Magnets, and of the Resistance of Wires," by J. H. Poynting, B.A., Demonstrator in the Physical Laboratory of the Owens College.

"On Aurin, Part II," by R. S. Dale, B.A., and C. Schorlemmer, F.R.S.

"Screw Propulsion," by Robert Rawson, Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society, Member of the Mathematical Society.

"On a Section of the Eucalyptus Globulous," by Professor W. C. Williamson, F.R.S.

"On the Oval Scars on the stems of the Carboniferous genus Ulodendron," by Professor W. C. Williamson, F.R.S.

The printing of Volume VI., third series, of the Society's Memoirs has been completed, and several of the above papers have been passed by the Council for printing in a new volume.

The Council still consider it desirable to continue the system of electing Sectional Associates, and a resolution on the subject will again be submitted at the annual meeting for the approval of the members.

The Librarian reports that the binding of the books is so far completed, that not much more money will be required, except for binding the periodicals as the current volumes are completed.

The number of Societies we correspond with is the same as last year, and no books of special interest have been added to the library.

On the motion of Mr. A. BROTHERS, seconded by Dr. JAMES BOTTOMLEY, the Report was unanimously adopted, and ordered to be printed in the Society's Proceedings.

On the motion of Mr. R. S. DALE, seconded by Mr. H. GRIMSHAW, it was resolved unanimously:—That the system of electing Sectional Associates be continued during the ensuing Session.

R. D. DARBISHIRE, it was resolved :—That as the Centena of the Society will occur in February, 1881, it be referr to the Council to consider the best means of celebrating i

On the motion of Mr. R. D. DARBISHIRE, seconded Mr. E. W. BINNEY, it was resolved :—

That the Society has heard with great satisfaction tl the Governors of Owens College have requested the Cour to prepare plans for buildings for a Museum of Natu History.

It would respectfully urge on the College the gı importance of completing such buildings with the l possible delay, because, as long as the College defers performance of the duty which it has undertaken in respect, the study of Natural History in this district, w its various branches are now pursued with great dilig by many able students, suffers under serious interruı and disadvantage.

It would remind the College that it has long be matter of great disappointment and regret, that with collections stored up as are now laid by in its rooms with a public so well qualified to appreciate the opportı and while so large an endowment has been accepted b College towards the support of a public Museum proper scientific and educational conditions, the n arrangements should have been so long postponed.

That the President be requested to forward a copy foregoing resolution to the Treasurer of the Owens C in the name of the Society.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:—

President.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.

Vice-Presidents.

EDWARD SCHUNCK, Ph.D., F.R.S., F.C.S.
EDWARD WILLIAM BINNEY, F.R.S., F.G.S.
HENRY ENFIELD ROSCOE, B.A., Ph D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, Ph.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A., F.R.S.

Treasurer.

CHARLES BAILEY, F.L.S.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
CARL SCHORLEMMER, F.R.S.
JAMES BOTTOMLEY, B.A., D.Sc., F.C.S.

MANCHESTER LITERARY AND

CHARLES BAILEY, TREASURER, IN ACCOUNT WITH THE SOCIETY

Dr. STATEMENT OF THE ACCOUNTS

	1878-9.						1877-8.					
	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d
1878.—April 1.												
To Cash in hand				1726	4	3				247	19	9
1879.—March 31.												
To Members' Contributions :—												
Arrears, 1877-8, 4 at £2 2s.	8	8	0									
New Member's Admission, 1877-8, 1 at £2 2s.	2	2	0									
,, ,, 1878-9, 6 at £2 2s.	12	12	0									
,, Half Subscription, 1877-8, 1 at £1 1s.	1	1	0									
,, Subscription, 1878-9, 6 at £2 2s.	12	12	0									
Old Members, 1878-9, 136, at £2 2s.	285	12	0									
				322	7	0				332	17	0
To One Compounder										26	5	0
To Two Associates' Sub. (for Library)	1	0	0				1	0	0			
To Sectional Contributions, 1878-9 :—												
Physical and Mathematical Section	2	2	0				2	2	0			
Microscopical and Natural History ditto	2	2	0				2	2	0			
				5	4	0				5	4	0
To Use of Society's Rooms to 31st March, 1878 :—												
Manchester Geological Society	30	0	0				30	0	0			
Manchester Scientific Students' Association	22	0	0				11	10	0			
Manchester Field Naturalists' Society	9	0	0									
				61	0	0				41	10	0
To Sale of Society's Publications				2	1	10				2	10	0
To Natural History Fund :—												
Dividends on £1225 Gt. Western Stock				60	1	8	68	16	9			
To Capital Fund :—												
Natural History Society's Donation							1500	0	0			
										1568	16	9
To Bank Interest, less Bank Postage, to 31st December, 1878				10	1	7				4	11	10
				£2186	19	11				£2229	14	4

	£	s.	d.	£	s.	d.	£	s.	d.
Compounders' Fund :—									
Balance in favour of this Account, April 1st, 1879							125	0	0
Natural History Fund :—									
Balance, April 1st, 1878	1611	18	7						
Dividends as above, in 1878-9	60	1	8						
	1671	19	10						
Expenditure on this Account, 1878-9	1675	8	11						
Balance against this Account, April 1st, 1879				3	9	1			
General Fund :—									
Balance against this Account, April, 1st, 1878	10	14	4						
Expenditure, 1878-9, as above	503	1	0						
	513	15	4						
Receipts, 1878-9, as above	400	14	5						
Balance against this Account, April 1st, 1879				113	0	11	116	10	0
Balance in favour of the Society							£8	10	0

PHILOSOPHICAL SOCIETY.

FROM 1ST APRIL, 1878, TO 31ST MARCH, 1879, WITH A COMPARATIVE FOR THE SESSION 1877-1878. Cr.

	1878-9.						1877-8.					
1879.—March 31.	£	s.	d.	£	s.	d.	£	s.	d.	£	s.	d.
By Charges on Property:—												
Chief Rent	12	13	6				12	15	2			
Insurance against Fire	12	17	6				13	7	6			
Property Tax	3	10	10				2	2	6			
Repairs, Whitewashing, &c.	3	14	0				2	0	2			
				32	15	10				30	5	4
By House Expenditure:—												
Coals, Gas, Candles, and Water	17	0	11				18	17	9			
Tea and Coffee at Meetings	18	18	10				18	14	6			
House Duty	6	7	6				6	7	6			
Cleaning, Brushes, Sundries	4	17	9				6	2	7			
				47	5	0				50	2	4
By Administrative Charges:—												
Wages of Keeper of Rooms	57	4	0				57	4	0			
Postages and Carriage of Parcels	20	7	2				26	17	4			
Attendance on Sections and Societies	14	11	6				15	14	0			
Stationery and Printing Circulars	12	12	6				8	9	3			
				104	15	2				108	4	7
By Publishing:—												
Printing Memoirs	50	9	6				74	4	6			
Printing Proceedings	57	11	0				63	15	0			
Wood Engraving and Lithographing	14	11	0				6	3	3			
Editor of Memoirs and Proceedings	50	0	0				50	0	0			
				172	11	6				194	2	9
By Library:—												
Binding Books	78	10	6				35	2	6			
Books and Periodicals	50	4	6				11	2	6			
Assistant in Library	12	4	0				11	6	0			
Old Memoirs of the Society							1	12	0			
Geological Record for 1876	0	10	6				0	11	2			
Palæontographical Society for 1879	1	1	0				1	1	0			
Ray Society for 1877, 1878, and 1879	3	3	0									
				145	13	6				60	15	2
By Natural History Fund:—												
Investment—£1225 Great Western Railway Stock	1489	18	0									
Brokerage on ditto	7	9	0									
Legal Charges	41	12	0									
Works on Natural History	36	9	11									
Grant to Microscopical and Natural History Section for Books	100	0	0									
				1675	8	11				60	0	0
By Balance				8	10	0				1726	4	2
				£2186	19	11				£2229	14	4

1879.—April 1.—To Cash in Manchester and Salford Bank £8 10 0

April 8, 1879, Audited and found correct,

(Signed) JAMES BOTTOMLEY,
R. S. DALE.

ARTHUR W. WATERS, F.G.S., was read:—

On February the 9th, 1876, I wrote you a letter, which was published in the Proceedings of the Literary and Philosophical Society, giving an account of the Naples Zoological Station, and as the Mittheilungen aus der Zoologischen Station zu Neapel shows that it has expanded since then into a more important establishment, I send a few supplementary particulars gathered from this first volume.

Since the date of my last the Station has received a small steamer for dredging purposes, partly through the liberality of the Berlin Academy, which voted 18,000 marks for the purpose, and the Prussian Government 6,000 marks. Dr. Dohrn has also made a more satisfactory arrangement with the Italian Government, who granted him the lease of the land for thirty years, and have now extended the term to ninety years.

The accomodation has been increased so that now, tables for 26 naturalists can be furnished, and the staff is very materially larger, there being now 25 constantly employed about the station. The increase has taken place in all departments, though the most material is in the scientific staff, which has been doubled. Dr. Berthold has charge of the botanical section, and Dr. Paul Mayer has the task of seeing after the zoological collections, and some other changes and sub divisions have been made, but Dr. H. Eisig from whom the naturalists who have already visited the Aquarium have received material assistance, still represents Dr. Dohrn in the detail control of the entire establishment.

The addition of the steamer has caused changes among the engineers, and necessitated several more boys about the place. These boys are much more important than would at first thought be supposed, for an intelligent lad soon learns to distinguish the animals which the specialists are investigating, and after a time knows the scientific names of a large proportion of the animals brought in, and in fact will soon have a wider superficial grasp than most of the naturalists, and able to quickly "spot" the animals required. The eldest boy (Torillo) when I was there had devoted himself diligently to increasing his knowledge, and was able to give valuable assistance, and I see he has had more important duties assigned to him, and Dr. Dohrn speaks of the increasing knowledge of the other boys.

The first volume of Mittheilungen contains several valuable papers. The continuous observations of Mr. Schmidtlein on the habits of the animals in the Aquarium bring out much that is interesting, and such observations are much needed in all aquaria. Dr. Dohrn discusses the zoological position of the Pycnogonidae. Dr. P. Mayer has in each part a paper on Carcinological studies, and in the second he follows up Mr. Bullar's investigations on the Isopoda, and supports most of his conclusions, showing the curious hermaphroditism of the group, the young being active males, while as they grow older most of the male organs wholly or partially disappear, in connection with which the female organs are then developed. Dr. Falkenberg gives a synopsis of the fucoids of the bay of Naples.

Most but not all of the papers in these two parts are by the staff, but the number of papers giving the result of studies made in the Station by naturalists is now very considerable, and is spread over the periodical literature of all scientific Europe.

As the institution now lays itself out more and more for supplying material to naturalists, prepared according to their instructions, the list with prices which is given at the end of the second part, will be of service to anyone requiring such aid.

This is much the largest zoological station, and may be looked upon as to a great extent the parent of others now springing up in many directions. Quite recently the Triest zoological station has also published three parts of a volume, containing a series of most valuable results attained there. There is also the station at Roscoff, which supplies the "Archives de zoologie experimentale" of M. Lacaze-Duthiers with many important papers. There is a more or less permanent station on one of the islands near Denmark, and it promoter are carried out, this will only be one of a series of stations confederated together in the southern hemisphere.

The extension of the Naples station has of course materially added to the expense, and as it is now so generally felt that biological science is very largely indebted to Dr. Dohrn's indefatigable perseverance, it is to be hoped that he will receive all the support he expects.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Annual Meeting, April 7th, 1879.

CHARLES BAILEY, Esq., in the Chair.

The Treasurer's Account and Report of the Council were read and passed.

The Election of Officers for the ensuing year then took place as follows:

President.

C. BAILEY, F.L.S.

Vice-Presidents.

THOMAS ALCOCK, M.D.
H. A. HURST.
A. BROTHERS, F.R.A.S.

Treasurer.

T. H. BIRLEY.

Secretary.

J. COSMO MELVILL, M.A., F.L.S.

Council.

W. C. WILLIAMSON, F.R.S.
JOSEPH BAXENDELL, F.R.A.S.
JOSEPH SIDEBOTHAM, F.R.A.S.
R. ELLIS CUNLIFFE.
JOHN BOYD.
JOHN BARROW.
E. W. BINNEY, F.R.S., F.G.S.
W. BOYD DAWKINS, F.R.S., F.G.S.

List of Members and Associates of the Section :—

Members.

Alcock, Thomas, M.D.
Bailey, Charles, F.L.S.
Barratt, Walter Edward.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Bickham, Spencer H., Jun.
Binney, E. W., F.R.S., F.G.S.
Birley, Thomas Hornby.
Boyd, John.
Brockbank, W., F.G.S.
Brogden, Henry.
Brothers, Alfred, F.R.A.S.
Cottam, Samuel.
Coward, Edward.
Coward, Thomas.
Cunliffe, Robert Ellis.
Dale, John, F.C.S.
Dancer, Jno. Benjamin, F.R.A.S.
Dent, Hastings Charles.
Derbyshire, R. D., B.A.
Dawkins, W. Boyd, F.R.S., F.G.S.
Deane, W. K.
Higgin, James, F.C.S.
Hurst, Henry Alexander.
Latham, Arthur George.
Martin, Sidney Trice.
Melvill, J. Cosmo, M.A., F.L.S.
Moore, Samuel.
Morgan, J. E., M.D.
Nevill, Thomas Henry.
Nix, E. W., M.A.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S., F.L.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist., Owens College.
Wright, William Cort.

Associates.

Becker, Wilfrid, B.A.
Hartog, Marcus M., B.Sc., F.L.S.
Kimmins, C. W.
Labrey, B. B.
Linton, James.
Peace, Thos. S.
Percival, James.
Plant, John, F.G.S.
Rogers, Thomas.
Stirrup, Mark, F.G.S.
Tatham, John F. W., M.D.
Young, Sydney.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION

OF THE

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY,

IN ACCOUNT WITH THOMAS H. BIRLEY, TREASURER.

Dr.

		£	s.	d.
1878.				
May 13.	—To B. Quaritch, Conchologia Iconica	115	0	0
Nov. 4.	— ,, J. B. Dancer, additions to Microscope	3	10	0
,, —	,, James E. Cornish, periodicals to July, 1878	2	2	4
,, —	,, Charles Simms, circulars to March, 1878	3	3	0
Dec. 12.	— ,, H. T. Stainton, Zoological Record, 1878	1	0	0
,, 27.	— ,, J. Von Voorst, Ibis, 1879	1	1	0
1879.				
March 10.	— ,, Subscription to Parent Society	2	2	0
,, 14.	— ,, B. Quaritch, Conchologia Iconica	9	19	3
,, —	,, W. Roscoe, two postages	4	2	2
,, —	,, James Collins	0	7	6
,, —	,, J. E. Cornish	0	5	0
		£142	12	3

Cr.

		£	s.	d.
1878.				
April 8.	—By Balance in Bank	108	14	10
—	,, Subscriptions:—			
	Five arrears at 10s	2	10	0
	Thirty-eight Annual at 10s	19	0	0
1879.				
March —	,, Interest allowed by Bank	0	6	6
—	,, Balance due to the Bank	12	0	11
		£142	12	3

Examined and found correct,

7th April, 1879,

ROBERT ELLIS CUNLIFFE,
JOHN BOYD.

MR. HURST gave a list of the Leguminosæ of the Riviera collected by JOSEPH SIDEBOTHAM, ESQ., in the winter of 1877–78.

Argyrolobium Linnæanum. Walp.
Genista Germanica. L.
„ sericea. Wulff?
Spartium junceum. L.
Cytisus albus. Link. a J.S.
„ hirsutus. L.
Ononis minutissima. L.
Anthyllis tetraphylla. L.
„ vulneraria. γ. rubriflora. L.
Medicago littoralis. Rhde.
„ minima. L.
„ orbicularis. All. Very fine.
„ maculata. Will.
Melilotus officinalis. Desr.
Trifolium stellatum. L.
„ montanum. L.
„ patens. Schreb.
„ scabrum. L.
Dorycnium suffruticosum. Vill.
„ hirsutum. D. C.
Lotus ornithopodioides. L.
„ edulis. L.
Psoralea bituminosa. L.
Astragalus monspessulanus. L.
do. Imperfect.
Scorpiurus subvillosa. L.
Coronilla Emerus. L.
„ valentina. L.
„ minima. L.
„ varia. L.
Arthrolobium scorpioides. L.

Ornithopus compressus. L.
Hippocrepis unisiliquosa. L.
Onobrychis. Resembles O. equidentata in foliage.
Vicia hybrida. L.
Ervum nigrescens. L.
Lathyrus sativus. L.
,, setifolius. L.
,, ? imperfect.
Orobus tuberosus. L.

MR. HURST likewise read a communication from Pv PLANTA on the Grasses of Egypt, enumerating as follows those found:—

Graminées d'Egypte.

Andropogoneæ.

Imperata cilindrica (Hoat) M. Tell, Sembaleven. Piraymidea.
Saccharum Ægyptiacum (Wild). Sicile. Rosette.
Sorghum halepense (Pers). M. Ramle.
Andropogon annulatum (Del). (de M. Letourneaux). Æ. Damenhour.

Paniceæ.

Panicum turgidum (Del). Æ. Mokatham.
Panicum crus galli (L). G. Rosette.
Panicum numidianum (Lam.) (muticum Forsk.) Æ. Rosette.
Panicum cruciforme Sibth. M. Rosetta.
Setaria verticillata (Pers). M. Ramle.
Pennisetum dichotomum (Del). Æ. Mokattam.

Phalarideæ.

Phalaris paradoxa (L). M. Piramides Sembaleven.
Phalaris aquatica (L). M. Gabari.

Phleicæ.

Crypsis Schœnoides (Lam). M. Ghizah, Mandara.

Agrostideae.

Polypogon monspeliense (Deof). M. Gabari.
Agrostis pungens (Zehreb). M. Ramle.
(Sporobolus pungens) (Kunth).

Agrostis verticillata (Vill). M. Ramle.
Agrostis spicata (Del). Æ. Ramle.

Stipaceæ.

Stipa tortilis (Desf). M. Ramle.
Aristida plumosa (L). Æ. Mokattam.
Milium vernale (Rehb). M. Alexandria.

Arundinaceæ.

Arundo isiaca. (Del). Æ. Rosette.
Phragmites communis (L). G. Rosette.
Amophila arenaria (LR). (de M. Letourneaux). G. Mandara.

Chlorideæ

Cynodon Dactylon (Pers). G. Ramle.
Dactyloctenium Ægyptiacum. Sicile. Ramle.

Agrostideæ.

Aira minuta. M. Ramle.
Avena neglecta (Savi). (Trisetum neglectum, RS) M. Ramle.
Avena hirsuta (Wood). Sicile. Ramle.
(Avena sterilis L. var. hirsuta)
Avena arundinacea (Del). de M. Letourneaux. Æ. Mandara.

Festucaceæ.

Kocleria phleoides (Pers). M. Alexandria.
(Lophocloa phleoides (Will).
Schismus calycinus (Wood). Schismus marginatus (Beau).
Festuca calycine (L). M. Ramle.
Poa annua (L). G. Alexandria.
Poa cynosuroides (Vahl). Æ. Piramides.
Eragrostis megastachya (LK). M. Ghizeh.
(Briza eragrostis) (L).
Chrysurus aureus (Pers).) (Lamarkia aurea) (Mclh). M. Ramle.
Dactylis repens (Desf). (Dactylis littoralis) (Wilde).
Festuca fusca (Del). Æ. Sembeleven.
Festuca divaricata (Desf). Æ. Ramle.
Bromus confertus (M.B) M. Mex.
Bromus rubens (Del). M. Ramle.
Bromus madriteusis (L). Ramle.
Bromus polystachyos (DC). M. Ramle.

a result considerably remote from the correct value. Also when the disc was placed inside the solution at the depth given by theory, it seemed too dark. Next, a comparison was made by looking through the cylinders at external white surfaces. Standard solution 4000 in 500 cub. c. of water, length of column 8·4.

A	B	C
6000	5·1	6588

Thus the result is considerably different from the real value. Also both experiments concur in giving too high a value. The theoretical depths moreover were tried with external surfaces, and seemed slightly too great. My next experiments were made with solutions containing 2400 and 1600 of sulphate of copper. The discs being inside the results were as follows: standard solution 1600 in 500 cub. c., depth of disc 8·3.

A	B	C
2400	5·6	2371

The number under B was the result of seven trials. The value under C is a fair approximation to the real value. I also tried external surfaces with these solutions. Standard solution 1600 in 500 cub. c., length of column 8·4.

A	B	C
2400	6·2	2161

somewhat further from the correct value than we might expect. I also tried the theoretical length of column; with external discs it appeared a little too light. The results obtained with the last standard solution are inconsistent with the results obtained with the first. In the latter case the results are too low, while previously they were too high. Errors of observation arising from imperfect perception of colour, from imperfection of instruments and unfavourable conditions of light (for many of the experiments were made during the winter months, sometimes on omy unfavourable days) would no doubt contribute to this result, yet allowing for all these, there seemed to be some other cause. In my last paper I mentioned that an ammonaical solution of copper, when largely diluted, became turbid, and that to carry out the experiment an additional quantity of ammonia was necessary. This small quantity was added at hazard, as I did not think it would have any influence on the result. It seemed to me afterwards to be a point worth examining in connection with the above experiments. Two solutions were made, each containing 5 cub. c. of the previously mentioned copper solution with 245 cub. c. of water. The solutions were placed in similar cylinders—to one of the cylinders more ammonia was added: it appeared perceptibly darker than the other. Hence it appears that the excess of ammonia has some influence on the result. I presume that ammonio-sulphate of copper has a tendency to be decomposed by water, and that some change is effected even before it becomes obviously marked by the formation of a turbidity; moreover it seems likely that the excess of ammonia has the power to counteract this property of water, and to restore the original compound. Two solutions were made, the bulk of each being 545 cub. c., one containing 4000 of copper sulphate, along with an additional 20 cub. c. of ammonia, the other containing 6000 of copper sulphate with 30 cub. c. of additional ammonia. The comparisons were made in new cylinders graduated to millimetres. An experiment with white surfaces, external, gave the following results:

Standard solution 4000 in 545 cub. c. of water, length of column 23 cm.

A	B	C
6000	15·5	5955

thus the result is very near the real quantity. I also took shorter lengths of the standard solution, namely 18 cm., 13 cm., and 8 cm., the corresponding lengths of the other solution were 12·4 cm., 8·5 cm., and 5·3 cm. Reduced to 23 cm.

of the standard, the lengths would be 15·8, 15, 15·2, numbers not far removed from 15·5, which was got by observation. I repeated the experiment with fresh solutions, the bulk of the liquid being 500 cc. Standard solution 4000 in 500 cub. c., length of column 21·2.

A	B	C
6000	14·1	6000

The number under B is the theoretical quantity, and was the mean result of four trials. Also with shorter columns of the standard liquid, namely 15·2 cm., 10·2 cm., 5·2 cm., the corresponding lengths of the stronger liquid gave similar tints. At the same time lengths differing a little from the theoretical would also satisfy. With these solutions I also tried an experiment with discs inside. Standard solution same as last:

A	B	C
6000	12·3	6894

The number under B was the mean of 15 trials, thus the result with discs inside was not so good as with discs outside. Also 14·1 cm. the theoretical depth when tried seemed slightly too dark with discs inside. I repeated the experiment with a solution containing 2400, using one containing 1600 as a standard. In preparing these solutions to the stronger I added 12 additional cub. c. of ammonia, and to the weaker 8. With external white surfaces the results were as follows:—standard solution 1600 in 500 cub. c. of water, length of column 21·2 cm.

A	B	C
2400	13·8	2458

The number under B was the result of fifteen trials. Thus we get a good approximation to the real quantity. With discs inside the results were as follows. Standard solution same as last:

A	B	C
2400	12	2824

The number under B was the mean of twelve trials. The value got with discs inside is not so good as with discs outside. Also the theoretical depth 14·1 cm. when tried seemed to be slightly too great. From the foregoing experiments it would appear that with considerable additions of ammonia the results with discs outside were much improved. Why the results were not equally good with discs inside may, I think, be accounted for, and will be considered further on. I also compared the solution containing 4000 with the one containing 1600. Standard solution 1600 in 500 cub. c., length of column 21·2.

A	B	C
4000	8	4240

The number under B was the mean of eight trials. The theoretical length was 8·5; with the standard solution on the left hand this length seemed to give a similar tint, but with standard solution on right it seemed a little darker; and now I remarked for the first time, or, if I had previously remarked it, had not thought it worthy of notice, that even an apparently so trivial circumstance as altering the positions of the cylinders from right to left had a perceptible influence in the determination of colour. I afterwards made some experiments to test this. Next I compared solutions containing 1600 and 6000. Standard solution 1600 in 500 cub. c., length of column 21·2.

A	B	C
6000	5·1	6682

The number under B was the mean of eight trials. The result is not so satisfactory as the others. I also tried these solutions with white surfaces inside. Standard solution same as last:

A	B	C
6000	3·9	8697

A widely remote result, and much further from the real value than with discs outside, also a disc placed in the solution at the depth assigned by calculation seemed too dark. It seemed possible to me that a more satisfactory result than this experiment had yielded might be obtained. The excesses of ammonia used in the experiments were nearly proportional to the quantities of sulphate of copper in solution; but if we regard water as an agent whose tendency is to diminish the intensity of the colour, and ammonia as an agent whose tendency is to restore the colour, it would seem reasonable that the ammonia should be proportional to the water. The difference of the excesses of ammonia in the last two solutions was large, being 22 cub. c. I prepared fresh solutions, one containing 4 cubic c. of the copper solution with 30 cub. c. of additional ammonia and sufficient water to make 500 cub. c. The other solution contained 15 cub. c. of the copper solution with 30 cub. c. of additional ammonia, and sufficient water to make 500 cub. c. The quantities of the copper solution taken should correspond to 1600 and 6000 of copper sulphate; to guard against imperfect measurements from the burette; I also weighed the solutions. The 4 cubic centimetres weighed 3·9854 grams, and the 15 cubic centimetres weighed 14·99 grams. The ratio of the volumes is 3·75, and the ratio of the weights is 3·761, so that the error of measurement would be but small.

With discs outside the results of experiments were as follows. Standard solution 1600 in 500 cub. c. of water, length of column 21·2.

A	B	C
6000	6	5653

The number under B was the result of eight trials; also the standard solution was on the left hand. With the standard solution on the right the results were

A	B	C
6000	5·4	6283

The number under B was the mean result of eight trials. In one case the value got by experiment is too high, and in the other too low. The theoretical length is 5·65, and of the theoretical length it seemed to give the required tint when the standard solution was on the left; when the standard solution was on the right it seemed a little darker. The mean of the two values previously obtained is 5968, which is near to the real value. From the above experiments it seems that when excesses of ammonia are added, very fair approximations may be obtained by colorimetry to the quantity of copper sulphate in solution, the white surfaces being external. Also that with white surfaces internal the results are more remote. In my last paper I stated that when there was much difference between the standard solution and the one to be compared with it, the discrepancies when internal discs were used were considerable. Then I was using very small quantities of colouring matter. From these experiments where the colours were intense and the quantities of colouring matter used considerable, a similar conclusion follows. The reason for this is not far to seek, and it also suggests a correction that must be applied to the formula when the white surfaces are inside. A white disc inside a column of coloured liquid looks darker in colour than a white disc outside, placed a few inches below a column of the same length. This may be tried by looking through different columns, or through the same column, and having inside a white disc of smaller diameter than the cylinder; the inside disc will then appear surrounded by a rim of lighter colour. When the disc is internal, it is evident that the light which illuminates it has previously passed through the solution, so that we are looking not at a white disc, but at a coloured disc through a coloured solution. Some allowance must be made in the calculation for this coloration of the disc. The formula $qt = q't'$ is applicable to the case in which the surfaces are outside; to adapt this formula to the case when the discs are inside, suppose x to be the length of the column of fluid external and an internal surface, then the formula would be $q(t+x)=q'(t'+x)$. To find the value of x experimentally, I took a solution containing 2400 and sunk a small disc in it until the inside and outside colour seemed the same; for the outside the length of column was 22·5; for the inside 17·7, this being the mean of eight trials. The difference is 4·8. With a solution containing 1600, a white surface outside, with length of column 21·2, seemed to give the same colour as white surface inside with length of column 16·2. The difference is 5. I also tried to get the value by the following combination. A solution was taken, containing 2400 with disc inside, and compared with a solution containing 1600 with a disc outside; length of column in latter case was 21·2, in the former case a column 17·5 seemed to give a similar colour. From the formula $q(t+x)=q't'$ the resulting value of x would be 5·2. I also tried the following combination. Solution containing 2400 and disc outside was compared with solution containing 1600 with disc inside—the mean of eight trials gave length of column 17 in the latter case, equivalent to 21·2 in the former; from the formula $1600\ (17+x)=2400\times 21{\cdot}2$, the resulting value of x is 4·3—finally we get for the approximate value of x, taking the mean of the four determinations, $x=4{\cdot}8$ The experimental determination is not easy, but the value obtained gives better results when we use it in the formula; for instance, on a former occasion with discs inside, when a solution containing 1600 and length of column 21·2 was used as a standard, and there was compared with it a solution containing 2400, the length of column was 12; from the uncorrected formula the result is 2824; from the formula

$$q'(12+4{\cdot}8)=1600(21{\cdot}2+4{\cdot}8)$$

the resulting value of q is 2476, which is not far from the proper value. When the fluids compared differ much in strength, the value of the correction will probably vary. It will be better not to have the difference large, whether the discs be external or internal, for when the differences are large any errors in the determination of the lengths of the columns have a greater effect on the calculated result. I also tried to make a rough estimate of the length of the cylinder, which might be covered without any perceptible darkening of the disc. I found that a black cloth cover investing the cylinder might be drawn down until it was about 3·5 or 3·2 cm. from the disc; this would vary with the dimensions of the window and the relations of the cylinder to it. Also the length given is less than the value of x previously deduced; but it ought to be so, for the light from a vertical window to illuminate a horizontal disc must pass obliquely through the solution. It would also follow that parts of the disc more remote from the window are darker than parts nearer, hence if the cylinders are of moderate radius, either the discs should be small and should be kept with their centres moving along the axes of the cylinders, or in the case of larger discs the determination of colour should be made by a comparison of similar parts of the discs. In the estimation of colour it is also not a matter of indifference, when we are given any tint as a limit, how we approach that limit. Suppose we have two cylinders, α and β, full of coloured liquid, that in α being the darker. Now pour out from α until the colour seems the same as in β; before reaching the theoretical division sight will fail to discern any difference of colour. Now if we proceed cautiously, as we approach the limit there will be a natural hesitation and tendency to stop, and it seems likely that in most cases in obedience to that feeling we shall stop with a column a little too long. Now suppose we start with the cylinder α empty, and pour fluid into it; as we again approach the limit cautiously we shall again have a tendency to stop, and inasmuch as before reaching the limit we pass likely to take a column of fluid too short. In my own case I have noticed this on several occasions; for example, in trying to estimate a particular colour the mean of seven trials made by pouring out from the cylinder gave 14·1 as the length of the column, the mean of seven trials made by pouring into the cylinder gave 13·7 as the length, and with discs inside the mean of six trials made by moving the disc from below upwards gave 12·5 as the length of column, while moving from above downwards the mean of six experiments gave 11·7. In trying to estimate a colour it seems to me that it would be well to approach the limit by both ways, and then take the mean of the results.

In a previous part of this paper I stated that altering the position of the cylinders made a little difference in my perception of colour. I made some experiments to try this. I took a solution containing 2400 and poured from the cylinder on the right hand into the cylinder on the left hand; the columns ought to be equal. The mean of nine trials gave length of column on right hand 11·2; length of column on left hand 10·61. In these experiments, the judgment was made using both eyes. I next tried using one eye only—with the right eye the results were, length of column on right hand 10·86, length of column on left hand 11·09; these numbers were the mean of nine trials. With the left eye alone, the results were, right hand cylinder 10·78, left hand 11·01, being the mean of nine trials; thus using one eye only, the results are nearly the same in both cases; they also tend to make the right hand a little less, thus reversing the case of two eyes. These experiments were made in a room with a small window facing the south. I afterwards repeated the experiments, using two eyes, in another room having a window of larger dimensions and facing the north. A solution was used containing 1600, the mean of nine trials gave right hand cylinder 11·16, left hand 10·55, nearly the same results as I got before. Why I should have this tendency to make one column a little larger than the other I do not know; possibly it may be some peculiarity of vision confined to myself. In the course of my experiments I have also noticed the following curious phenomenon, and this repeatedly, when working with solutions coloured with bichromate of potash, and with ammonio-sulphate of copper; look steadily with one eye, say the right, through the solution at a white surface, after the lapse of about a minute suddenly turn the head so as to bring the left eye close over the cylinder, then the colour will seem more intense than it did with the right; having looked with the left eye for about a minute, bring again the right eye suddenly close over the cylinder, and the colour will seem more intense than it did with the left, and so on alternately. It would seem as if the first impressions of colour on the eye were the stronger, and as if there were a gradual and imperceptible decrease in intensity—perhaps alterations in the aperture of the pupil may contribute to this. Another matter for consideration in colorimetry is the nature of the incident light. On some occasions we have the light from a blue sky; on other occasions the sky is invested with clouds of various depths of grey, or sometimes tinged by the sun with a variety of tints, from yellow to red; while the light of the sun itself is frequently yellow or orange. All these variations of light are likely to have some influence in our judgment of colour, especially when the tints to be compared are light. Of the disturbing influence of colour in the incident light, anyone may convince himself by comparing yellows on a morning when the sky is enveloped in a yellow fog. In some experiments which I made with bichromate of potash during such fogs, I found it much more difficult to decide at what depth equality of colour was effected; the disc in the stronger solution could be moved through a very considerable range without any change of colour being perceived. A similar result happened when I hung up yellow screens and tried to make determinations of colour behind them; also when looking at light yellow external surfaces, differences in the lengths of the columns failed to give any difference in tint, although when looking at white external surfaces they did so. But in quantitative determinations of matter by colorimetry, the excellence of the results require sensible variations in colour when we alter slightly the length of the column; hence when the incident light is tinged with the colour we wish to determine the advantage of the method is diminished. Such a consequence may also be deduced from the formula which I obtained in my last paper. For suppose white light to consist of yellow, blue, and red (as far as the reasoning is concerned we might have considered it also composed of green, red, and violet, as some physicists do). Let I denote the incident white light, and B, Y, R the intensities of blue, yellow, and red necessary to produce white light, so that we may write:

$$\mathrm{I} = \mathrm{B} + \mathrm{Y} + \mathrm{R}.$$

Let there be two solutions containing q and q' of yellow colouring matter, and t and t' the corresponding lengths of columns; then the intensity of the light transmitted through one cylinder will be

$$(1 - mqt)\mathrm{Y} + (1 - m_1qt)\mathrm{R} + (1 - m_1qt)\mathrm{B},$$

m denoting the amount of yellow light absorbed by a unit layer, and m_1 the amounts of red and blue absorbed by a unit layer. Also the light transmitted by the other cylinder will be

$$(1 - mq't')\mathrm{Y} + (1 - m_1q't')\mathrm{R} + (1 - mq't')\mathrm{B}.$$

Since both cylinders are of the same colour these expressions will be equal. m will be less than m_1 because the transmitted light is yellow; let $m = m_1 - \mu$. Then we shall have

$$\mathrm{I}(1 - m_1qt) + \mu qt\mathrm{Y} = \mathrm{I}(1 - m_1q't') + \mu q't'\mathrm{Y})\ldots\ldots\ldots.(\mathrm{A})$$

The expression on the right hand denoting the light transmitted through one cylinder, and the expression on the left cylinder. Each expression consists of two terms, the terms of the form $I(1-m_1qt)$ denotes the white light transmitted, the term of the form μqtY denotes the excess of yellow; this term we may call the effective yellow, for it is the only portion which produces the sensation of colour. Now suppose the light before passing through the cylinders to pass through a yellow screen, suppose the composition of the incident light after transmission through the screen to be

$$\rho Y + \rho_1 B + \rho_1 R,$$

ρ being greater than ρ_1, say $\rho = \rho_1 + r$, so that the composition of the light may be written

$$\rho_1 I + rY,$$

rY being the effective yellow after passing through screen. After using the screen the left hand expression of A would become

$$I\rho_1(1 - m_1qt) + Yr - Yrm_1qt + \rho_1qtY\mu.$$

Since $qt = q't'$, if we substitute $q't'$ for qt in the last expression, we shall get the light transmitted by the other cylinder after using a screen; hence if the columns be adjusted so as to produce equality of colour with white light, they will still be in adjustment if the light should become tinged with yellow.

Let y_1 and y_2 denote the effective yellows in one of the cylinders with white incident light and with yellow light; then we shall have

$$y_1 = \mu qYt$$
$$y_2 = Yr - Yrm_1qt + Y\rho_1\mu qt$$

Now suppose the length of the column of coloured liquid to be altered a little so as to become $t + \delta t$, let δy_1 and δy_2 denote the alterations in the effective yellow in each case, then

$$\delta y_2 = (Y\rho_1\mu q - Yrm_1q)\delta t$$
$$\delta y_1 = Y\mu q\delta t$$

Hence

$$\delta y_2 - \delta y_1 = -\{Yrm_1q + \mu qY(1 - \rho_1)\}\delta t$$

Therefore δy is less than δy_1, that is to say, we shall not see when the light is tinged with yellow as when it is white; therefore the sensibility of the method is diminished. This may be put in another way. When the incident light is tinged yellow, the expression for the effective yellow after transmission through the cylinder is

$$Yr - Yqt(rm_1 - \rho_1\mu)$$

Suppose the term in brackets to vanish, then the expression for the effective yellow becomes Yr, which is independent of the quantity of colouring matter and of the length of the column.

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PROCEEDINGS

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LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 7th, 1879.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"On an extension of the Ordinary Logic, connecting it with the Logic of Relatives," by JOSEPH JOHN MURPHY, F.G.S. Communicated by the Rev. ROBERT HARLEY, M.A., F.R.S.

All logic deals with relation, and in this paper the common logic is treated as being that branch of the logic of relatives which deals with the relations of inclusion and exclusion.

The proposition of the common logic "All A is B" is here expressed by "A is included in B," or "A is an enclosure of B." The converse of this, as commonly stated, is "Some B is A;" but this is insufficient, because when reconverted it only gives back "Some A is B," and reconversion ought to give back the original proposition. The converse form here proposed is "B includes A," or "B is an includent of A."

When all M is A and all M is B, the conclusion is expressed in the common logic by "Some A is B." This syllogism is here expressed thus:—"A is an includent of M; B is an includent of M; therefore A and B are co-includents of M;" or if we drop the M, "A and B are co-includents."

When all A is M and all B is M, there is no conclusion recognised by the old logic; yet there is a valid conclusion, which here appears in the following syllogism:—"A is an enclosure of M; B is an enclosure of M; therefore A and B are co-enclosures of M;" or, by dropping the M, "A and B are co-enclosures." As an instance of this in expressing actual relations:—"The Duke of Wellington was an Irishman; Lord Palmerston was an Irishman; therefore the Duke of Wellington and Lord Palmerston were fellow-Irishmen;" or, by dropping the name of the particular nation, "The Duke of Wellington and Lord Palmerston were fellow-countrymen."

The relation of inclusion is here expressed by L, and its converse by L^{-1}. Co-inclusion, or the relation of one enclosure to another is expressed by $\frac{L}{L}$ or L^0; co-includence, or the relation of one includent to another by $\frac{L^{-1}}{L^{-1}}$ or $(L^{-1})^0$.

In this system, so long as the contrary is not stated, enclosure always means enclosure in the same includent, and includence means includence of the same enclosure. Consequently from the premises "A and M are co-includents; M and B are co-includents;" we have the conclusion that "A and B are co-includents." In the common logic, on the contrary, the premises "Some A is M; some M is B," yield no conclusion.

The fundamental canon of this system is that the enclosure of an enclosure is an enclosure; and conversely, that the includent of an includent is an includent. These are expressed by the equations $L^2=L$ and $(L^{-1})^2=L^{-1}$. The former of these is the canon of the old "Syllogism in Barbara;" the latter is the same read backwards. These equations express that the relation of inclusion and includence is transitive.

The relations of co-enclosure and co-includence are also transitive, and they are, moreover, invertible,—that is to say, if $A=L^0B$ or $A=(L^{-1})^0B$, then $B=L^0A$ or $B=(L^{-1})^0A$. These are expressed by the equations $(L^0)^{-1}=L^0$ and $([L^{-1})^0]^{-1}=(L^{-1})^0$ To express this in words:—every term

with zero index is equal to its own second po
to its own reciprocal. This is also true in ari
it is moreover generally true in arithmetic th
term has the value of unity, which is not true

The four terms L, L^{-1}, L^{0}, and $(L^{-1})^{0}$ w
give sixteen equations, whereof each is th
syllogism. Two of these have been stated
direct and the inverse expression of the canon
Of the sixteen syllogisms, fourteen are concl
to say, in fourteen cases, whereof the two ju
are examples, the two relations combine into
expressed by one of our four terms. Anoth
that given above where the middle term is “
this in the present notation is expressed
$M=L^{-1}B$, therefore $A=L^{0}B$; or, simply as a ca
tion and without using any but relative terms,
The forms of the two inconclusive syllogisms
and $(L^{-1})^{0}.L^{0}$.

The truth of the equations of this system do
on the interpretation of L as inclusion, but
transitiveness of the relation symbolized by
consequently remain true if L means, for inst
or cause; postulating in the latter case that
cause is a cause.

For the purpose of the present essay, relati
as analogous, though not closely so, to rati
terms to numerical coefficients. In logic, as
if one of the following four equations is tru
necessarily so.

$$\frac{A}{B}=L \qquad \frac{B}{A}=L^{-1}$$

$$A=LB \qquad B=L^{-1}A$$

where $\frac{A}{B}$ is taken to mean, in logic, any rel
of A to B.

The relation of exclusion is expressed by N:—"A is not B" is expressed by $A=NB$. Co-exclusion is expressed by N^0. Exclusion is not a transitive relation, and it is invertible; in other words, N is not equal to its own second power, and is equal to its own reciprocal. The only numerical coefficient which unites these two properties is negative unity. In logic the following equations are true:

$$\begin{array}{ll} N.N=N^0 & \qquad N^0.N=N \\ N.N^0=N & \qquad N^0.N^0=N \end{array}$$

That is to say

The excludent of an excludent is a co-excludent.	The co-excludent of an excludent is an excludent.
The excludent of a co-excludent is an excludent.	The co-excludent of a co-excludent is a co-excludent.

And the interpretations are similar if N means "not in relation with" or "not related to, as either cause or effect." These four equations are also true in arithmetic, if N is taken to mean negative unity and N^0 unity.

The PRESIDENT described a simple means for checking the oscillations of a telescope. It consisted of a leaden ring placed centrally about the axis of the tube of the telescope and attached thereto by three or more elastic caoutchouc bands. He had employed two of these rings for his telescope, one placed near the object glass, the other near the eyepiece. Their united weights were only one quarter of that of the telescope tube, but nevertheless they diminished the time required for the cessation of vibration to one sixth of what is was before their application.

Dr. E. SCHUNCK, F.R.S., exhibited some specimens of the colouring matters, &c., referred to in his paper "On the Purple of the Ancients," lately published.

Ordinary Meeting, October 21st, 1879.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Notes on some Fossils from the Iron Mines of Furness," by E. W. Binney, V.P., F.R.S., F.G.S.

In vols. VIII. and XII. of the Memoirs, and vol. VII. of the Proceedings of the Society are printed papers by me on iron ores and their origin. In the last I gave an opinion that the hæmatite ores of Furness were of the age of our Lower Lancashire Coal Measures, owing to specimens of carboniferous plants, especially the *Sigillaria vascularis*, having been found in them. In the Transactions of the North of England Institute of Mining and Mechanical Engineers, vol. XXVIII., part V., p. 234, Mr. T. D. Kendall, C.E., F.G.S., in replying to some remarks in a paper of his on the Hæmatite Deposits of West Cumberland, said, "With regard to the so-called vegetable remains mentioned by Mr. Binney, he had, since his paper was published, shown them to Professor Williamson, of Owens College, who pronounced them not to be plants at all." The chief evidence on which I based my opinion was a most beautiful specimen of *Sigillaria vascularis* shown to me many years ago by the late Mr. Bolton, of Swarthmoor, near Ulverston, exhibiting in a marked manner all the characters of that plant. Where this specimen now is I cannot tell, but I am pretty sure that neither Mr. Kendall nor the Professor is likely to have seen it. Other specimens presented to me by my friend the late Miss Hodgson, of Ulverston, from Water Blain Mine, near Broughton, were exhibited to the meeting at the time I read my paper, and the above-named gentlemen could not have seen them, for the simple reason that they have been me how Mr. Kendall can have exhibited the "so-called vegetable remains mentioned by Mr. Binney" to Professor Williamson.

On the table I have brought some of my specimens from Water Blain, and others from Cark, Lindal Moor, and different places in Furness, kindly lent me by Mr. Swainson, of the Poplars, Ulverston, so that the members may judge for themselves whether they are the remains of plants or accidental forms, notwithstanding that they are now converted into good hæmatite iron.

The stems of fossil plants found in our coal measures are all in an altered condition; some have been calcified, others silicified, and frequently ferrified, or converted into hæmatite iron, bisulphide, or a carbonate of the protoxide of that metal, and we have to determine them more from their external characters than from internal structure. On the table are exhibited specimens of fossil wood converted into peroxide of iron and silicate of alumina from the Smedley sandstone at Cheetham, near this city, a rock in the upper portion of the middle coal field. The vegetable origin of these have never been doubted, although little evidence is left of their external characters or internal structure. These red coloured specimens were first, probably, in a partly calcified state. The lime was afterwards dissolved out and the iron converted into a peroxide, the silica and the alumina remaining, and the fossils were thus altered into their present ochrey condition. The white specimens are composed of silicate of alumina and are mere pseudomorphs with probably slight traces of iron.

From notes made by me in 1836, the time I found the fossils, the ochrey and white ones were met with at the bottom of the sandstone in a stratum of soft argillaceous matter of a dark red colour, mixed with streaks of white. On clearing this away I observed a soft fibrous substance

resembling decayed wood, about two feet in le
inches in breadth, and varying from half to o
thickness. On the slightest force being applied,
off the stone underneath in a similar way that b
off the boles of decayed trees. · This singular co
not extend around the whole of the stem whic
bedded in the rock, but appeared only on the up
and reaching a short distance round the sides.
lying parallel to the stratification, but whether
compressed stem or the bark of an underlying
difficult to say, but at the time I was inclined to t
opinion.

Bischof, in his Chemical and Physical Geology,
42, says that specular iron is a very remarkable
material, and gives instances of fossil shells whi
entirely of crystalline laminæ of specular iron,
fibrous red iron ore was met with by G. Sandb
petrifying substance at a mine in the neighbo
Oberscheld, in Nassau. The same author also
these petrifactions are of no little importance in a
point of view, for they furnish altogether decisive
that specular and fibrous red iron ores are formed i
way, whether the oxide of iron occurs in vein
pseudomorph.

The same author also states that as the brow
in displacement-pseudomorphs, and the material f
mation can only be furnished by the soluble bica
iron, it is remarkable that such waters, though f
so occurring, have not oftener caused the petri
animal remains. However, according to Zippe
Mineral. 1843, p. 616), spathose iron ore occurs as
ing material of wood at the Postelberg in Bohemi
recently met with black oxide of manganese as th
ing material of an ammonite from the mines a
near Sargaus, in Switzerland.

Most of the specimens exhibited are of peroxide of iron, but Mr. Swainson's black coloured plants from Cark have been converted into protoxide of iron.

It is of course impossible for me to know what specimens Mr. Kendall may have submitted to Professor Williamson when that gentleman is said to have pronounced my specimens not to be plants at all, but I now bring before the society Mr. Swainson's and my own to speak for themselves. The former show the rhomboidal scars on the outside as well as the pith, and the internal and external radiating cylinders of *Sigillaria vascularis* from Lindal Moor. There are also several specimens of *Stigmaria ficoides,* the well-known roots of *Sigillaria,* exhibiting the rootlets of the plant coming from the main root in regular quincuncial arrangement from Cark, which Mr. Swainson has kindly intrusted me with. My own specimens from Water-Blain I own are not in such good preservation as those of Mr. Swainson, but most collectors of fossil coal plants would recognise them as *Lepidodendron, Stigmaria,* and *Calamites,* and their occurrence in any strata would be taken to prove the beds to be of carboniferous age, notwithstanding that Mr. Kendall does not believe them to be plants of any kind, but simply what is known as "ring ore."

As to the origin of hæmatite iron, nothing that has come under my observation has altered the opinion I expressed in my paper printed in vol. XII. (2nd series) of the Society's Memoirs, that the iron was primarily derived from volcanic sources, similar to that observed on the sides of Vesuvius in 1817, as a chloride; but how it was conveyed into the places where it is now found it is difficult to say in the present state of our knowledge. However, it is most probable that the ores of Furness came into the places where they are most found about the time the lower coal measures of Lancashire were deposited.

"On the 'Differential Calculus' of Du Bourguet," by Sir **James Cockle**, F.R.S., Corresponding Member of the Society.

In "Notes and Queries" for September 6, 1879 (5th S., vol. xii., pp. 182-3) I have given a bibliography of Du Bourguet's work* on the calculus (Paris, 8vo; vol. i., 1810; vol. ii., 1811). From that work (viz., from vol. ii., pp. 75-6) I translate the following article, premising that Du Bourguet's equation (at p. 75) is the well-known criterion of integrability:—

"361. 2°. Every equation between three variables which does not satisfy this (330), is not integrable, and consequently appears at first sight destitute of significance. Nevertheless M. Monge has demonstrated, in the *Memoirs of the Academy of Sciences of Paris* (year 1874), that such an unintegrable differential equation, between three variables, represents an infinite number of curves of double curvature possessing a common property. Besides, we shall observe that, in these non-integrable equations, this (330) gives a relation between the three variables, which, as it stands, or augmented, or diminished by a constant quantity, often satisfies the proposed; such is, for example, the equation

$$(y-z)dz+(z-y)dx+(x+a)dy=0 \quad \ldots \quad (a),$$

for which the equation of condition (330) is not satisfied, since it is in this case

$$z=x+y+a\,;$$

yet if we substitute this value of z in the proposed (a), we have the identical equation $0=0$.

"Again, let there subsist

$$\{1+\sqrt[3]{(z-y-x)}[1+a\sqrt[4]{(z-y-x)}-b\sqrt[3]{(z-y-x)}]\}dx$$
$$+[1+x\sqrt[3]{(z-y-x)}]dy-dz=0 \quad \ldots\ldots \quad (b),$$

for which the equation (330) becomes

$$z=x+y+\left(\frac{3a}{4b}\right)^{12} \quad \ldots\ldots\ldots \quad (c).$$

---

* I first saw a copy of this work in July last, when my brother, Dr. John Cockle, presented me with the two uncut volumes, bound in a printer's covering or binding of blue paper, and each with a white ticket on the back whereon the words "Du Bourguet. Calc. Diff. et Intégral" are printed, with the respective additions "Tome I." and "Tome II."

Thus the proposed (b) is not integrable, neither is it satisfied on therein substituting the value of z given by the equation (c); but if we cut off from this value the constant term, the remaining equation $z=x+y$ satisfies the proposed."

We may apply to solutions obtained as suggested in the above article the term "discriminoidal." But there exist unintegrable equations having single solutions not to be so obtained. Solutions of the latter class may be called "ultra-discriminoidal."

The equation (7) given by Mr. Robert Rawson at page 117 of his interesting communication "On Tertiary Differential Equations" (*ante*, vol. xvi., pp. 114-8) is very remarkable as well as very general. It belongs to a distinct species of discriminoidal solution. The reciprocal of the first factor of the dexter of Mr. Rawson's (8) is an integrating factor of his (7), provided that w is the product of a function of x into a function of y. An analogous property is possessed by every equation of the species to which Mr. Rawson's equation (7) belongs.

2, Sandringham Gardens, Ealing,
October 10, 1879.

PHYSICAL AND MATHEMATICAL SECTION.

October 14th, 1879.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

"On Colorimetry, part III.," by JAMES BOTTOMLEY, D.Sc.

In this paper I give the results of some furthur experiments to test the accuracy of the assumption that when light is transmitted through transparent coloured solutions, the length of the column multiplied by the quantity of colouring matter is constant if the colour is constant. In a communication which I made to the Society in April of this year, I gave the results of some experiments with ammonio-sulphate of copper, which appeared to indicate a failure of the law; but the failure was traceable to the decomposition of the salt by water, and better results were obtained when a suitable menstruum was employed. I was wishful to obtain some colouring matter which might be diluted with water without decomposition; it occurred to me that caramel would be a suitable body. I prepared some caramel by heating loaf sugar. The resulting dark brown vitreous mass dissolved entirely in water. In these experiments I also wished to see if the law would hold when one quantity was a considerable multiple of the other; also the quantities used are no longer mere traces. In order to avoid an ambiguous result from any difference in sensbility to colour of the two eyes, in making the determinations I used one eye only. The cylinders used in these and previous experiments were not specially made for colorimetric purposes.

At the bottom they were curved a little inwards. Measurements were taken from the summit of the curve. When in such cylinders we have short columns of fluid, the depth not being uniform, the colour is not uniform over the whole area as we look through the cylinder at an external white surface. Manifestly the colour at the sides is more intense than at the middle, but for purposes of comparison we must restrict our attention to the middle. It is not easy to confine attention to a limited portion of a coloured area so as to receive no impression from the remainder of the area without some provision. Hence it is necessary to limit the field of view at the bottom of the cylinder—this was done either by placing small porcelain discs on a black ground and holding the cylinder so that its axis passed through the centre of the disc—or still better by covering the bottom of the cylinder with a black external plate having a small hole (about quarter of an inch diameter) in its centre. With such a provision columns seemed in some cases to satisfy the experiment, which otherwise would have given the impression of too dark a colour. In these experiments I used a method for determining colours indicated in my last paper, regarding the proper colour as the mean of two sets of determinations, one set giving too great and the other too small values. Thus the determination of colour has some analogy with the method used by old geometers for determining areas bounded by curved lines; considering them as the limits of internal and external polygons. In these experiments A denotes the number of cubic c. of caramel solution mixed with water; B the length of the column; and C the number of cubic centimetres thence derived by calculation. In one experiment the mean of four trials for the greater limit gave 2·83 cm.; and the mean of four trials for the smaller limit gave 2·65. Hence the result will be as follows:—Standard solution contains 10 cub. c. of caramel in 500 cub. c. of water, length of column 21·2 cm.

A	B	C
80	2·74	77·4

The above result was obtained by using the right eye alone. I made another series of determinations, using the left eye alone. For the greater limit the mean of four trials was 2·85, and for the smaller limit the mean of four trials gave 2·58. Hence the result will be as follows—Standard solution same as last.

A	B	C
80	2·71	78·2

I next made some experiments with stronger solutions. For the greater limit the mean of four trials gave 2·78, and the mean of four trials for the smaller limit 2·68. Hence the results were as follows:—Standard solution 40 cub. c. of caramel solution in 500 cub. c. of water; length of column 21·2—observations made with right eye only

A	B	C
320	2·73	310·6

I also compared a solution containing 320 cub. c. with another solution containing 10 cub. c, the theoretical length was 0·65 cm., a column between 0·6 and 0·7 would satisfy, but the meniscus rendered the exact determination difficult. I also made a further experiment with the solution containing 40 cub. c.; one determination for the greater limit gave 5·7, and one determination for the smaller gave 5. Hence the results are as follows:—Standard solution 10 cub.

c. in 500 cubic c. of water, length of column 21·2 cm.—observations made with right eye.

A	B	C
40	5·35	39·6

Thus the result is very near.

The solutions of caramel ought not to be kept many days. After the lapse of twelve days some of the solutions were turbid and unfit for comparison, owing to the development of vegetable organisms. It seems very probable that even with large differences between the lengths of the columns and with larger quantities of colouring matter the relation qt=constant, is valid when the colour is constant. But suppose the colour to vary, what will be the connection between the quantity of colouring matter, the length of the column, and the intensity of colour? If q denote the quantity of colouring matter per unit of length, and t the total length, we have the relation $qt=c$ if the colour be constant; but if the colour vary, c will be a function of the transmitted light. Hence

$$c=f\,(\mathrm{T})$$

if T denote the transmitted light, therefore $qt=f(\mathrm{T})$ or as we may write it $\mathrm{T}=\phi(qt)$, the probable form of this function may be obtained as follows: suppose we have two perfectly transparent cylinders of unit area and a fluid of such a nature, that if in any portion of it we dissolve some colouring matter, on further addition of the fluid no decomposition takes place. Suppose we have a standard solution containing one unit of colouring matter per unit of volume. If the colouring matter remain constant in quantity, then the intensity of the light will be a function of the length of the column of fluid only, say $\psi(t)$, and if the length of the column of fluid remains constant, the intensity of light will be a function of the quantity of colouring matter only, say, $\phi(q)$. Suppose, now, in the cylinders which we may distinguish as A and B we pour a unit length of the standard fluid, then the light transmitted will be the same in both; hence we shall have $T=\psi(I)=\phi(1)$. Dissolve in A another unit of the colouring matter and make the column of the standard solution two units long in B, the colour will remain the same; hence we have $\psi(2)=\phi(2)$. If we dissolved three units in A and made B three units long, we should again find $\psi(3)=\phi(3)$, and generally $\psi(n)=\phi(n)$. If then we know $\psi(n)$ we shall obtain $\phi(n)$. For the intensity of light transmitted through a column n units long, Sir John Herschel has given an expression (to which I have referred in a previous paper) of the form

$$\Sigma a k^n$$

k being the intensity of light passing through a unit thickness, a the intensity of the incident light, and the summation having reference to the composite nature of light. This formula is given by Herschel in the "Encyclopædia Metropolitana," also in an article on the absorption of light by coloured media in the "Transactions of the Royal Society of Edinburgh." In neither of these works do I find the experimental confirmation of the formula. It appears to have been obtained *a priori*. If we assume its accuracy we shall obtain for $\phi(n)$ the expression ak^n, if we suppose we are dealing with homogeneous light; if we substitute q for n we shall obtain ak^q for the intensity of light which has passed through a unit length containing q units of colouring matter. We may now suppose the length to vary—for two units of length the expression will be $a(k^q)^2$ for three

$a(k^q)^3$ and for t units $a(k^q)^t$. Finally, if we suppose that there are various kinds of light, we have

$$T = \Sigma a k^{qt}$$

As a probable expression for the intensity of light passing through a column t units long and containing q units of colouring matter per unit of length. I think that in many cases where the relation $qt =$ constant fails, it may be traced to some decomposition having taken place, or to some change effected by light; for example, I commenced some experiments with ferricyanide of potassium, but as it did not prove a suitable salt for making experiments without some special precautions with regard to the action of light, I discontinued them. As I am not aware that any one has particularly noticed this darkening, a few remarks may be interesting. A standard solution was prepared containing 0·8 grms. in 500 cub. c. In the afternoon having occasion to use this standard for comparison with another, the result was not satisfactory owing to its transparency not being so perfect as when freshly made. On the following morning I made a fresh standard solution of the same composition; it differed from the old in being more transparent, and I thought that it had more of a greenish tint. This new solution being left on the table before the window, after a time became of diminished transparency; also on looking down into the cylinder a very faint red cloudiness was perceptible. I also compared a solution containing 3·2 grms. in 500 cub. c. which had been freshly prepared with a solution containing 6·4 grms in 500 cub. c. this solution had been prepared on the previous day and had been exposed to light during that interval. I found the length of the column indicated by theory decidedly too great; it occurred to me that the discrepancy was due to some action of light on the ferricyanide. About six o'clock in the afternoon I again compared these solutions; the theoretical length gave a colour which was still too dark, but the disparity of colour was not so marked as at first. The comparison was also disturbed a little by the slightly diminished transparency of the weaker solution.

I now prepared a fresh solution, containing 6·4 grams in 500 cub. c., thinking that when we work with solutions which vary gradually in colour we are apt to forget the initial condition. This new solution seemed quite different from the old one of the same strength. The latter was much darker and browner. So great was the difference that 9·1 cm. of the old seemed as dark as 22·5 of the new. To find whether the darkening was due to the action of light or to some intrinsic cause, I divided the newly made solution into two equal columns. One I left on the table before the window; the other I kept in a cylinder which was closely invested with black cloth. After the lapse of six hours I compared them. The one exposed was so much darker that 5 cm. of the exposed solution gave a tint as deep as 10·9 cm. of the unexposed. This observation was made on the Saturday. On midday of the following Monday, when I again compared them, the darkening had evidently increased, for 3 cm. of the exposed solution gave a tint about as dark as that furnished by 10·9 cm. of the unexposed.

Wishing to ascertain whether keeping in the dark would reverse the action of light, on Saturday, May 24th, I took a solution containing 6·4 grams in 500 cub. c. The solution having been prepared three days previously and darkened by exposure during that interval to light. The containing cylinder was closely invested with black cloth and kept in a dark closet. On the morning of the following Monday I thought that it appeared not quite so dark as at first, and on the evening of the same day I thought it a little lighter than in the morning. After keeping it in the dark for a week I found that it had become much lighter, and on June 4th, when I examined it again, it seemed nearly as light as a freshly prepared solution; there was, however, a minute quantity of precipitate.

From these results it is evident that in some cases special provision must be made to avoid needless exposure to light in quantitative determinations by colorimetry, or in studying the laws of the absorption of light passing through coloured solutions.

I also made some experiments with chromate of potash. This I thought a stable salt suitable for experiments. Nevertheless some of the results were not satisfactory when one cylinder contained a solution which was several times stronger than the other. For instance: a standard solution was made containing 0·8 grams in 500 cub. c. of water. Another solution compared with this gave the following results:—

A	B	C
6·4	3·7	4·5865

A repetition of the experiment gave nearly the same result, namely, 3·6 for the length of the column.

It occurred to me that possibly, when potassic chromate is diluted, there may be liberated a minute quantity of chromic acid, which would increase its absorbent power; this might be inferred from the greatly increased absorbent power imparted to the bichromate by the additional molecule of CrO_3. I therefore took the cylinder containing the standard solution used in the last experiment and divided its contents into two equal columns: to one I added a few drops of ammonia; this column became slightly but perceptibly lighter than the other, so that I have little doubt some change had been effected in the constitution of the dissolved salt; the hypothesis of the liberation of a little chromic acid is, I think, strengthened by the fact that a solution of the salt is of a deeper yellow than the undissolved salt. I think that probably a trace of carbonic acid in the water had liberated a little chromic acid.

To try what the effect of the addition of a little weak acid would be, I took a solution containing 1·6 grams in 500 cub. c. and divided it into two equal parts. To one I added a little extremely dilute sulphuric acid. The colour of this portion became decidedly deeper than that of the other. I also tried what would be the effect of the addition of a little ammonia to a strong solution, so I divided the solution containing 6·4 grms. in 500 cub. c. into two equal portions—one I treated with ammonia; this I thought a little lighter than the other, but the difference was very slight; this however we might expect, for any small change in intensity would be less noticeable in a strong solution than in a dilute one.

I now made some fresh experiments with chromate of potash, a little ammonia being added to both columns. The mean of four trials gave for the greater limit 3·35, and the mean of four trials gave for the smaller limit 2·18. Hence in 500 cub. c. of water; length of column 22·5 :

A	B	C
6·400	2·77	6·498

In this experiment I used the right eye. The theoretical length is 2·81, and the above result is therefore a near approximation.

With the same solutions, on the following day (June 26th) the results were not so favourable; the mean of eight trials gave 2·57 cm. as the length of the equivalent column, the left eye being used in the determinations. On the next day (June 27th) I repeated the experiments with these solutions, using the right eye; the mean of four trials gave 2·3. To each solution I added 5 cub. c. of ammonia, and repeated the experiment; the mean of four trials gave 2·13 as the result. These differences of results are probably due to some internal changes in the coloured fluids.

I may also take this opportunity to correct two numerical errors in the sixth volume of the Society's Memoirs. Page 262, line 3, for 3·2 read 2·2; and on page 264, line 2, for 15 read 50.

Ordinary Meeting, November 4th, 1879.

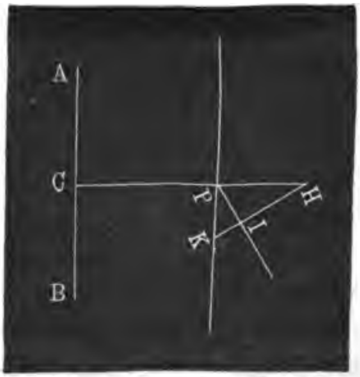
J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Screw Propulsion," Part II., by **Robert Rawson**, Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society, Member of the Mathematical Society.

7. The articles in Part I. have been principally devoted to the consideration of the normal velocity of the propeller blade, an element which chiefly, if not entirely, regulates the *thrust* of the screw in the direction of its axis; but recent experience in screw propulsion seems to point to another resistance arising from the friction between the water and the surface of the propeller blades usually called *skin resistance.* This skin resistance, therefore, appears to depend, in some way or other, upon the velocity of the rubbing surfaces. (See a paper by Rankine on the Mechanical Principles of the Action of Propellers.—Transactions of the Institution of Naval Architects, 1865, page 21.)

Hence, the necessity of determining the velocity of the *element* (a) in a plane perpendicular to its normal line.

8. This may be done as follows:



Let HP, in the annexed figure, be perpendicular to the plane ACP. Now the velocity (rw) acts in the direction of HP, resolve this velocity perpendicular to the normal PI, it will become $rw\sin\nu$ in the direction HI and in the plane HPI, which is evidently perpendicular to the plane ACP. Again, resolve the velocity (v), which acts in the direction PK, perpendicular to the normal PI, it becomes $v\sin\alpha$ in the direction KI and in the plane PIK. It is not difficult to show that the cosine of the angle between these component velocities will be correctly represented by the following formula

$$\text{Cos. KIH} = -\frac{\cos\alpha\cos\nu}{\sin\alpha\sin\nu} \qquad \text{(6)}$$

Therefore, the velocity of the element (a) in a tangent plane, that is, in a plane perpendicular to its normal line at P, is the resultant velocity of $rw\sin\nu$, $v\sin\alpha$ acting in directions making the angle KIH, and, by the ordinary formula for calculating the resultant it becomes equivalent to the formula

$$\sqrt{r^2w^2\sin^2\nu + v^2\sin^2\alpha + 2rwv\cos\alpha\cos\nu} \qquad \text{(7)}$$

9. The above result may be obtained in another way, viz. find the resultant of rw and v in the plane HPK, then resolve this resultant parallel and perpendicular to the normal line.

The formula (7) admits of the following form, easily obtained, viz.:

$$\sqrt{r^2w^2 + v^2 - (rw\cos\nu - v\cos\alpha)^2} \qquad \text{(8)}$$

10. The point P moves with a velocity $\sqrt{r^2w^2 + v^2}$, and describes the common helix whose radius is ν and pitch ($2\pi v$) upon (w).

11. Taking 6080 feet to represent a knot or nautical mile,

$$\therefore\ v = \text{feet per second} = K \times 1{\cdot}688$$

where K is the knots per hour.

If, n = number of revolutions of screw per minute,

$$\therefore\ u = \text{angular velocity} = \frac{\pi n}{30}$$

On the Slip of the Element (a) of the Propeller Blade.

12. Let it be supposed that the *element* (a) rotates in water perfectly at rest, and also that its motion is the same as the motion of the nut of a screw. This condition is evidently fulfilled when the normal velocity of the *element*

(a) is equal to zero. Let u', v' be the angular velocity, the velocity in the direction of the axis AB, respectively, which will produce the above described effect, then

$$ru'\cos\nu - v'\cos\alpha = 0.$$

$$\text{From which, } \frac{v'}{u'} = \frac{r\cos\nu}{\cos\alpha} \ldots\ldots\ldots\ldots\ldots\ldots \quad (9)$$

It now remains to find the distance H′ traversed along the axis of the *element* (a) viz. AB during one revolution, then

$$\text{Time of one revolution} = \frac{\text{space}}{\text{velocity}} = \frac{2\pi}{u'}$$

$$\therefore H' = \text{time} \times \text{velocity} = \frac{2\pi v'}{u'} = \frac{2\pi r\cos\nu}{\cos\alpha} \ldots\ldots\ldots\ldots \quad (10)$$

Again, to find the distance H traversed along the axis AB during one revolution of the *element* (a), when it moves with an angular velocity (u) and a velocity (v) in the direction of its axis, then

$$\text{Time of one revolution} = \frac{\text{space}}{\text{velocity}} = \frac{2\pi v}{u}$$

$$\therefore H = \text{Time} \times \text{velocity} = \frac{2\pi v}{u} \ldots\ldots\ldots\ldots (11)$$

13. The quantity H′ is usually described as the measure of the speed of the *element* (a) of the propeller blade, and H as the measure of the speed of the vessel propelled.

When H′ is equal to H, then the *element* (a) of the propeller is said to move as fast as the vessel.

When H′ is less than H, then the *element* (a) is moving faster than the vessel.

When H′ is greater than H, then the *element* (a) is not moving so fast as the vessel.

The quantity (H′—H) has been usually designated by naval architects the *slip* of the *element* (a).

The value of this *slip* is readily found from equation (10) and (11) as follows;

$$H' - H = 2\pi\left\{\frac{r\cos v}{\cos\alpha} - \frac{v}{u}\right\} \ldots\ldots\ldots\ldots \quad (12)$$

cases of the quantities (H′—H) having been negative, giving rise to what is known to practical naval architects as *negative slip*. *Hannibal*, a screw steamer of 3136 tons, speed 7·999 knots per hour, 64·87 revolutions per minute, propeller 17ft. diameter, pitch 12ft. 6in., length 2ft. 1in., indicated horse power 1071, had a negative slip of ·601 knots per hour. *Plumper*, 3129 tons, speed 6·627 knots per hour, 111 revolutions per minute, propeller 8ft. 8½in. diameter, 6ft. 0¾in. pitch, 11⅞in. length, indicated horse power 135, had a negative slip of ·591 knots per hour.

Many other instances of this peculiar kind of slip have been recorded in the history of screw propulsion; but, its occurrence has generally provoked, amongst those interested in the progress of naval architecture, considerable discussion as to its real cause, some affirming its impossibility by an exclamation somewhat as follows:—how can the cart go faster than the horse, &c.? while others have endeavoured to see a cause for it, either in the flexibility of the propellor blade when it is subject to pressure, or, in the state of the water in which the propeller revolves. The history of this discussion is curious, and many of the leading events in it are recorded in the pages of the transactions of the society of Naval Architects. With respect to the importance attached to the subject of slip, it may be said that its introduction into the investigations of screw propulsion has been regarded by many in the light of an untoward event; inasmuch as its diminution does not necessarily imply an increase in the speed of the ship produced by the same effective horse power of engines. And, moreover, the advocates of "slip resistance" would be sorely perplexed to assign properly the element called slip in a propeller with a variable pitch.

15. With a view, therefore, of freeing the question of slip as much as possible from the elements which appear to me

to render difficult, to some minds, the right apprehension of its action in regard to the motion of the vessel, it will be necessary to consider here two ships being propelled by a screw propeller, placed in two hypothetical positions as follows.

Ship A. This ship is propelled by a screw revolving in a position astern, that is, out of the wake of the ship in still water.

Ship B. This ship is propelled by a screw revolving in a position astern, that is, in the wake of the ship in water which flows in the direction of the ship with a velocity V.

With respect to the ship A, the slip of the *element* (*a*) is computed on the above principal in Arts. 12 and 13, and is correctly represented by the formula

$$H' - H = 2\pi\left\{\frac{v\cos\nu}{\cos a} - \frac{v}{u}\right\} \ldots\ldots\ldots\ldots\ldots\ldots(13)$$

with a screw revolving as above described there would be, indeed, just ground for surprise if (H′—H) was experimentally observed to be either a negative quantity, or zero.

16. With respect to the ship B. Put u', v' for the angular velocity, the velocity of the *element* (*a*) in the direction of the axis, repectively, in order to produce the normal velocity equal to zero.

The slip will be here computed by the element (*a*) *moving in such a manner that its normal velocity is zero.*

Now, having regard to V, there results from formula (4) Art. 6, the equation

$$ru'\cos\nu - (v' - V)\cos a = 0.$$

$$\text{From which, } \frac{v'}{u'} = \frac{V}{u'} + \frac{r\cos\nu}{\cos a} \ldots\ldots\ldots\ldots\ldots\ldots (14)$$

Next to find the distance H′ traversed along the axis AB during one revolution of the *element* (*a*) of the propeller blade—

$$\text{Time of one revolution} = \frac{\text{space}}{\text{velocity}} = \frac{2\pi}{u'}$$

$$\therefore H' = \text{time} \times \text{velocity} = \frac{2\pi v'}{u'} = 2\pi\left\{\frac{V}{u'} + \frac{r\cos\nu}{\cos a}\right\} \ldots\ldots (15)$$

during one revolution of the *element* (a) when it moves with an angular velocity (u), and, a translatory velocity (v) along the axis.

$$\text{Time of one revolution} = \frac{\text{space}}{\text{velocity}} = \frac{2\pi}{u}$$

$$\therefore \mathrm{H} = \text{Time} \times \text{velocity} = \frac{2\pi v}{u}$$

Therefore, by subtraction, there results

$$\mathrm{H}' - \mathrm{H} = 2\pi \left\{ \frac{\mathrm{V}}{u'} + \frac{r\cos\nu}{\cos a} - \frac{v}{u} \right\} \ldots\ldots\ldots\ldots\ldots \quad (16)$$

17. In formula (16), although the form and dimensions of the propeller, together with the resistance to motion, might be such as to make the quantity

$$\frac{r\cos\nu}{\cos a} - \frac{v}{u}$$

equal to zero or even negative. Still, it would be surprising to find by experiment that $(\mathrm{H}' - \mathrm{H})$ in equation (16), was either negative or even zero.

In all probability the quantity

$$\frac{\mathrm{V}}{u'} + \frac{r\cos\nu}{\cos a}$$

would be considerably in excess of the ratio v upon u, providing the quantities V and u' were correctly measured by experiment.

Hitherto, when negative slip has manifested itself in actual experiment, as in the case of the Plumper and Hannibal, it has done so only on the assumption, which is by no means a correct one, of $\mathrm{V} = 0$.

The foregoing investigation appears to explain clearly the perplexing mystery of negative slip, and it may be added that the arguments and conclusions with respect to negative slip advanced in these pages are not, in the slightest degree invalidated by the circumstance of having used the *element* (a) of the propeller blade instead of the propeller itself. The reason of this is, that the slip of the *element* (a) is exactly the same as the slip of the ordinary screw pro-upon *cosa* is a constant quantity for every point on the screw blade, viz., the pitch of the screw.

It is no part of my design here to investigate the law which connects the distance (x) of the screw from the stern of the vessel with the velocity V of the water in which the screw revolves, the admission of a quantity V is sufficient for my present purpose.

It may, however, be observed that the velocity V varies from the velocity (v) of the ship at the stern, to the velocity zero at a point which is the limit of the wake of the ship. Those who are interested in and investigating this subject would do well to consult two papers printed in the Transactions of the Society of Naval Architects, by Professor Osborne Reynolds. The first paper is entitled "On the Effect of Immersion of Screw Propellers," (see Transactions of Ins. N. Architects, 1874, page 188). The second paper is entitled "On the Unequal Onward Motion in the Upper and Lower Currents in the Wake of a Ship, &c., (see Tran. of I. N. Architects, 1876).

18. The most recent experience in screw propulsion is given in a paper by James Wright, Esq., Vice-President of the Institution of Naval Architects, &c. The title of this paper is "The Steam Trials of Her Majesty's Ship Iris;" it was read and discussed at the Institution of Naval Architects, April, 1879.

Here, in a four-bladed common screw, a negative slip is recorded, which increases from 1·57 to 5·33 per cent, while the speed diminishes from 16 to 8 knots per hour, and the I.H.P. diminishes from 7,503 to 755, and the revolutions per minute diminishes from 91 to 43. This circumstance is passed over in silence by Mr. Wright, who very justly directs the attention to the fact that a two-bladed screw is a more effective propeller than a four-bladed one of the same dimensions.

Twin-screw Propellers" was read and discussed at the Institution of Naval Architects, April 11th, 1878, by W. H. White, Esq., Assistant Constructor to the Navy, Member of Council, in which is recorded two cases of negative slip. The Invincible, with twin-screw, displacement 5,563 tons, 70·8 revolutions per minute, 4,832 I.H.P., with four screw blades whose dimensions are 16ft. 2in. diameter, 20ft. 9⅞in. pitch, speed 14·093 knots per hour, gave a negative slip of 1·89 per cent. The Alexandra, twin-screws, 9,432 tons displacement, 64·2 revolutions per minute, 8,615 I.H.P., with four screw-blades, 21ft. diameter, 22ft. 2¾in. pitch, speed 15 knots per hour, gave a negative slip of 6·5 per cent.

October 13th, 1879.

CHARLES BAILEY, F.L.S., President of the Secti
Chair.

"On the Means by which Hydra swallows it
M. M. HARTOG, M.A., B.Sc., F.L.S.

The current idea is that Hydra swallows by tak
in its tentacles and turning tentacles and all into i
However, the part played by the tentacles cease
the mouth comes in contact with the food. The
slowly stretches itself over the food in a way th
some extent the manner in which a serpent "gets
prey, or in which an automatic stocking might st
on to the foot and leg. No care seems to be take
to present the easiest point for deglutition, an
mostracan may be swallowed sideways, for ins
far are the tentacles from co-operating in the act
are usually reflexed away from the food; occasio
ever, they are swung forward for a moment
mass as if to ascertain how much remains to be s

If the prey be at all bulky, immediately after
act is completed the body cavity is everywhere
on the stretch; but after a short lapse of tim
contracts forcibly along the long axis, so that th
taining the food is globular, supported on a slend
with a slender apical process bearing the tentac
the hypostome.

Mr. R. ELLIS CUNLIFFE called attention to a
Mr. H. J. Carter on the classification of the Spo
exhibited a series of slides prepared by Messrs
the original specimens.

rare bivalve mollusc Panopæa Aldrovandi, from Faro Island, Algarve, S. Portugal, a species almost confined to this isolated locality. He also exhibited, for sake of comparison, specimens of the British Panopæa Norvegica and various Saxicavæ, and sketched the distribution of the six species comprised in the genus Panopæa, the range being a remarkably wide one.

Dr. ALCOCK read a paper on Lagena striata and its allies, and exhibited many beautiful drawings illustrative of this genus of foraminifera.

Mr. LINTON distributed specimens of Helix lapicida (L.) from Matlock, Derbyshire.

Ordinary Meeting, November 18th, 1879.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President,
in the Chair.

The President noticed the lamented death, since the last meeting, of Professor James Clark Maxwell, an event which, having occurred in the prime of his life and in the midst of his usefulness and his splendid researches, was felt most severely by the Society and the whole scientific world.

E. W. Binney, V.P., F.R.S., said: On Tuesday, the 11th instant, at 5.30 p.m., my son informed me that he was walking in Trees Street, Cheetham Hill, and on looking towards the East he observed a very brilliant meteor in the S.E. It appeared to be considerably larger in size than the planet Jupiter, and gave out as much light as the half moon and travelled slowly to the N.N.E., where it disappeared. The colour of the meteor was white with a tinge of blue, and it left a track of the same tint. At its greatest elevation it reached the altitude of the planet Mars, and it appeared less in size as it travelled northwards and disappeared in the N.N.E. The time he observed it from first to last was about eleven seconds. The sky at the time was partially clouded, and the meteor was only observed at intervals and not along its entire course.

"Recording Sunshine," by David Winstanley, F.R.A.S.

So far as I have seen there is in use at present but one form of apparatus which effects an automatic registration of the duration and the times of sunshine, and that is the instrument of Campbell, in which a sphere of glass is so disposed as to burn a piece of wood or paper by the concentration of his rays when the sun may chance to shine. During the past few years I have devoted some attention to this matter and devised a number of appliances having the same object for their end but differing materially both in their construction and in the manner of their use from the apparatus I have named.

One of these, with your permission, I will now describe.

It is an arrangement which places a lead pencil on a sheet of paper and writes down therewith when and for how long the sunshine lasts.

It consists essentially of a differential thermometer with a long horizontal stem, in which latter is contained throughout the greater portion of its length some fluid intended to operate by its weight. This thermometer is attached to a scale beam or some equivalent device which also carries the pencil by means of which the record shall be made.

The whole is so arranged that in its normal state it rests gently—upon that side to which the pencil is *not* attached—on an embankment provided for that end.

Close beneath the pencil point a disc of metal rotated at the proper speed carries a paper dial whereon marks and figures are engraved corresponding with the hours at which the sun may shine.

When using this instrument I have it enclosed within a box which permits one bulb only of the thermometer—that most distant from the clock—to be affected by the radiance of the sun, which when it shines expands the air contained therein, forces the fluid along the tube and by altering the equilibrium of the beam brings some portion of its weight to bear upon the pencil point, and so the record is commenced.

When the sun becomes obscured, the air expanded by his rays contracts, the fluid in the tube returns, the normal equilibrium is restored, and the pencil ceases to produce its mark.

In the instance of the instrument I use the stem of the thermometer is 18 inches long and the eighth of an inch or thereabouts in bore.

Mercury in consideration of its weight is the fluid I employ, and in conjunction with it some sulphuric acid is enclosed, because of the mobility which is thereby gained. I am aware that in these circumstances mercuric sulphate is very slowly formed, but after two years' lapse of time no inconvenience has been caused thereby and the mobility of the mercury remains.

The bulbs of the thermometer are two inches in diameter or thereabouts, and that they may be more rapidly affected the glass thereof is thin. Both are blacked, and the one intended to receive the radiance of the sun projects above the box in which the apparatus is contained into a dome of glass.

"On some Notices in Classical Authors of the Action of Sunlight on Purple Dye," by JAMES BOTTOMLEY, D.Sc., F.C.S.

At a meeting of the Society on October 21st, Dr. Schunck exhibited to us some of his specimens of purple extracted from shell-fish, and called attention to the remarkable action of light in developing the colour. At the time I remembered having seen some passage relative to a supposed action of light upon the colour, showing that the ancients had some obscure notions of the matter. This seems to me additional evidence of the identity of the colour obtained by Dr. Schunck with the famous purple of the ancients. The passage which I had in my mind is a note by Paley on the following verses of the *Helena* of Euripides:

> κυανοειδὲς ἀμφ' ὕδωρ
> ἔτυχον ἕλικά τ' ἀνὰ χλόαν
> φοίνικας ἁλίῳ πέπλους
> αὐγαῖσιν ἐν χρυσέαις
> ἀμφιϑάλπουσ' ἔν τε δόνακος ἔρνεσιν.

Paley's comment is ἁλίῳ. So Herm. for ἁλίω or ἁλίου. See above on v. 170. Musgrave shows from Pollux, i. 49 that the chemical effect of the sunlight on garments dyed with the sea-purple is to refresh and heighten the hues, *Hippol.* 125: ὅϑι μοί τις ἦν φίλα φάρεα πορφύρεα ποταμίᾳ δρόσῳ τέγγουσα, ϑερμᾶς δ' ἐπί νῶτα πέτρεας εὐαλίου κατέβαλλε. From this property of the sea-purple Æschylus calls it κηκὶς παγκαίνιστος, *Agam.* 933, "capable of being entirely renewed when faded." The passage of Musgrave (and of Pollux) referred to by Paley is as follows:

"Φοίνικας—πέπλους. Purpureas vestes. Has enim soli exponere mos erat ad renovandum tincturæ splendorem. Pollux, lib. i. sect. 49, χαίρει δὲ ἡλίῳ ὁμιλοῦσα τῆς πορφύρας ἡ βαφὴ καὶ ἡ ἀκτὶς αὐτὴν ἀναπυρσεύει, καὶ πλείω ποιεῖ καὶ φαιδροτέραν τὴν αὐγὴν." The meaning of this passage I take to be—"The purple dye delights in association with the sun, and the solar beam reddens it and makes more intense and more dazzling its splendour."

Mr. CHARLES BAILEY, F.L.S., exhibited specimens of *Ophioglossum vulgatum*, L., var. *β. ambiguum*, Coss. et Germ., which he had collected in July last on damp sandy ground at the foot of the sandhills, on the land side, one mile west of Dyffryn railway station, between Harlech and Barmouth, in Merionethshire. This plant has been figured on plate 46 of "British Ferns," by the late Sir William Hooker, and the Welsh specimens agree well with this figure, though generally smaller in size. The variety *ambiguum* was originally detected more than twenty years ago in the neighbourhood of Paris, and was found, shortly afterwards, in one of the numerous "laiches" at Arcachon. It was first noticed as a British plant by Dr. J. T. Boswell, who detected it in the Orkney Islands, and Mr. Curnow has recently distributed specimens, through the Botanical Exchange Club, from the Scilly Islands.

The Dyffryn locality is therefore a connecting link between the extreme stations of western Europe, from the Orkneys in the north to Arcachon in the south. This variety, in general appearance, is much more like *Ophioglossum Luisitanicum*, L., but the var. *ambiguum* produces its fertile frond at the end of July, while *O. Luisitanicum* fruits, in Guernsey, five or six months earlier. M. Durieu de Maisonneuve finds a separating character in the spores, which are finely tuberculated in *O. vulgatum*, and smooth in *O. Luisitanicum.*

"On the Origin of the word Chemistry," by CARL SCHORLEMMER, F.R.S.

Chemistry as a science is first mentioned* by Julius Maternus Firmicus, a native of Sicily, and procurator under Constantine the Great. He wrote at about 336 a work on Astrology, which has been preserved only in a defective state, and is commonly known by the name of *Mathesis.*

In this work he states that by observing the position of the moon, in respect to certain heavenly bodies or constellations, at the hour when a child is born, its future inclinations can be predicted. He continues: *Et si fuerit haec domus Mercurii, Astronomiam. Si Veneris, cantilenas et laetitiam. Si Martis, opus armorum et instrumentorum. Si Jovis, divinum cultum et scientiam in lege. Si Saturni, scientiam alchimiae. Si Solis, providentiam in quadripedibus. Si in Cancero, domus sua, scientiam dabit omnium quae exeunt de aqua.*†

Other editions of this work have also *scientia alchimiae,*‡ but Vossius informs us that in the manuscripts it is *chimiæ.*§ He says: *Alchimiæ scientiam nominat Firmicus, lib. III., cap. XV. Ita quidem editum ab Aldo, sed in chirographis est chimiæ.*

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* Kopp, Beiträge zur Geschichte der Chemie, 43.

† Julius Firmicus de nativitatibus; Ed. Simon Bivilaqua. Venice, 1497.

‡ Ed. Aldus Manutius, Venice, 1499; Ed. Nicolaus Brucknerus; Bâle, 1533.

§ Etymologicon linguæ latinæ; Amsterdam, 1695.

Athanasius Kircher also states that the manuscript in the library of the Vatican has *chymiæ* and not *Alchymiæ*.*

Firmicus does not give any explanation of this term. However another writer, who probably lived at the same time, if not earlier, explains it. Zosimus, the Panopolite, according to Georgios Synkellos, a writer of the ninth century, states that χημεία (or χυμεία, as some manuscripts have) meant the art of making gold or silver.†

The curious passage in which this word occurs is the following:—

"The sacred Scriptures inform us that there exists a tribe of genii, who make use of women. Hermes mentions this circumstance in his Physics; and almost every writing (λογος), whether sacred (φανερος) or apocryphal, states the same thing. The ancient and divine Scriptures inform us that the angels, captivated by women, taught them all the operations of nature. Offence being taken at this, they remained out of heaven, because they had taught mankind all manner of evil, and things which could not be advantageous to their souls. The Scriptures inform us that the giants sprang from their embraces. *Chema* is the first of their traditions respecting these arts. The book itself they called *Chema;* hence the art is called *Chemia.*"

It is not difficult to trace the origin of this myth. We find it first in Genesis, chap. vi.: "And it came to pass, when men began to multiply on the face of the earth, and daughters were born unto them, that the sons of God saw the daughters of men that they were fair, and they took them wives of all which they chose."

"There were giants in the earth in those days; and also after that, when the sons of God came in unto the daughters of men, and they bare children to them, the same became mighty men, which were of old, men of renown."

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* Kopp, loc. cit. 9.

† Thomson's History of Chemistry, 5.

taught women all the secrets of nature.* That one of these is the art of making gold and silver is however first mentioned by Zosimus. Other Greek writers use the word Chemia or Chymia in the same sense; in print we find it first in the Lexicon of Suidas, who lived in the eleventh century and defines χημεία as "the preparation of gold and silver."

All the earlier Greek writers who mention this word were in close connection with the university of Alexandria; from this it has been inferred that the artificial preparation of the noble metals was first attempted in Egypt.

That country was conquered by the Arabians in 640. Here they made undoubtedly their first acquaintance with chemical science; they prefixed their article to the Greek name and thus introduced the terms: Alchemy, Alchimy or Alchymy.

The origin and meaning of these terms have often been discussed. Plutarch states that the old name of Egypt was χημία; that it was so called on account of its black soil, and that the same word designated the black of the eye. From this the conclusion has been drawn, that chemistry originally meant the science of Egypt, or the black of the eye being the symbol of darkness and mystery, chemistry was the secret or black art. But alchemy has never been called the black art, a name which was exclusively reserved for magic or necromancy.

It has also been stated that the name was derived from the Arabic *kema,* to hide, while others have maintained that the founder of our science was Cham or Ham, the son of Noah, or an Egyptian king with the name of Chemmis. It has further been suggested that the name of the science was derived from χέω, to melt; or from χυμός, juice or liquid.

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* Kopp, loc. cit. 4.

To this it has been objected that the original spelling was *χημεία* and not *χυμεία*, which, although Hermann Kopp, the great historian of chemistry, inclines to this view, has not yet been proved satisfactorily. Humboldt believes that the latter word got into some manuscripts by a mistake of the transcriber, and continues: "Alchimy commenced with the metals and their oxides, and not with the juices of plants." This objection, however, cannot be maintained at all, because vegetable juices or, at least, substances designated by their names, are mentioned by the older alchemists as the most potent substance by which transmutations could be effected.[*]

Some time ago my friend Professor Theodores called my attention to an interesting paper on this subject, published by Professor Gildemeister,[†] in which he maintains the derivation of the word chemistry from *χυμός*. According to him *kîmiyâ* in Arabic does not originally have an abstract meaning, and is the name, not of a science, but of a body by which, or rather by a substance obtained from which, the transmutation of metals is effected; it is synonymous with *iksîr*. Alchemy, as a science, was called: The preparation of *kîmiyâ* or *iksîr*, also the science of the preparation of *kîmiyâ* or, more shortly, science of *kîmiyâ*. In the Arabic Lexicon (Qâmûs) *al-iksîr* is explained by *al-kîmiyâ*, and the latter again by the former, or by any medium which, applied to a metal, transports it into the sphere of the sun or the moon, *i.e.*, converts it into gold or silver.

Even to this day the word is used in the concrete sense; Kotschy[‡] relates that the pasha of Nicosia talked much of flowers, chiefly *kimia*, a plant having the property of converting metals into gold.

The later writers, however, called the science shortly *al-kîmiyâ* and retained the term *al-iksîr* (elixir) for the transmuting medium or the philosopher's stone. This latter word is identical with *ξήριον*, as the writers of the Alexandrine school called the philosopher's stone,[*] while the same name was employed by the physicians for a healing powder, used for sprinkling over wounds, *i.e.*, a desiccative powder (from *ξηρός*, dry).[†]

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[*] Kopp, loc. cit. 76.

[†] Zeitsch. deutsch. morgenländ. Ges. XXX., 634.

[‡] Petermann. Geog. Mitth. VIII., 294.

Now the correlate to dry is moist or liquid, *χυμός*, and from this is derived *χυμεία*, a moist substance corresponding to *λιϑεία*, a material formed of *λίϑος*, or *κεραμεία*, the occupation with *κέραμος*.

Ibn Khaldûn, who lived in the 14th century, says that from the philosopher's stone a liquid or a powder might be prepared called *iksîr*, which, when thrown on molten copper converted it into silver, and molten silver into gold. In opposition to its etymology the word is here used for a liquid, because at that time *kîmiyâ* no longer meant the transmuting substance, but the science of transmutation, and explains why to-day we may understand by elixir a liquid.

We also find that the philosopher's stone is often called the red tincture, from *tinguo*, to moisten.

It appears, therefore, very probable that the name of our science is derived from *χυμός*, and the proper spelling would therefore be *Chymistry*, as the "Times" newspaper for a long time insisted upon. As however this derivation has not yet been proved quite satisfactorily, the time-honoured term *Chemistry* will remain in use, and I think be retained even if it should be shown that *χυμεία* was the original spelling.

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[*] Kopp, loc. cit. 209.

[†] Zosimus calls the substance by which copper is tinged yellow or converted into brass: *τὸ διὰ τῆς ϑουθίας ξήριον*, a powder prepared by means of tutia; now tutia (zinc oxide) is still to-day used in medicine as a desiccative.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 10th, 1879.

Charles Bailey, F.L.S., President of the Section
in the Chair.

Professor **Boyd Dawkins**, F.R.S., brought before the notice of the Section a map on which was represented the geography of the British Isles in the Meiocene Age. The land extended northwards by way of the Faeroes and Iceland to Greenland on the one hand, and to Spitzbergen on the other, and is now represented by the area included by the 400 fathom line. This land barrier offered a means of free communication both to Europe and North America, by which both plants and animals were able to migrate from the one to the other. It explains the many species common to Meiocene Europe and America, such as the Sequoia, the Redwood, Magnolia, Tulip tree, and Swamp Cypress, and others. The climate of the Arctic region was then temperate, but not so warm as in the preceding Eocene age.

"Additional Note on Hydra," by **Marcus M. Hartog**, M.A., B.Sc., F.L.S.

Since my last paper I think I have found the clue to the false idea referred to. A Hydra that had swallowed a morsel larger than itself disgorged, as frequently observed, on my attempting to take it up for examination. On finding it half an hour after, three of its tentacles were turned into its digestive cavity, whence they were successively and slowly withdrawn. As the mouth closes but slowly after disgorging, I imagine the swallowing them to have been accidental; and a similar phenomenon carelessly observed may well have given rise to a false interpretation.

These hairs are very simple in form and so minute that they can only be glimpsed with the $\frac{1}{8}$″ objective. They are planted in depressions of the integument and are circular in section, the basal part thicker and tapered towards the middle (what is termed biscuit-shaped by the German histologists), the terminal part longer and ciliiform. Their importance is shown by their constancy in position, and by the bilateral symmetry of their distribution. They are found on both the dorsal and ventral surfaces of the abdomen, but I have only succeeded in seeing them on the dorsal surface of the cephalo-thorax, where they are most numerous in the frontal region. The pair on the tergum of the last abdominal segment are easiest to find; and it was after seeing these that I looked for them elsewhere. I have found them in both Cyclops and Canthocamptus, belonging to distinct families, whence I conjecture that their presence may be general in the order. Similar hairs implanted in pits have long since been demonstrated in Insecta, and possibly add to the delicacy of touch required by flying and swimming animals to enable them to thread their way through obstacles.

"On an Undescribed Acinetan," by the same.

The animal in question was found on the ventral surface of Cyclops gigas (especially females), on and about the bases of the oral appendages. In size it comes near Podophrya Cyclopis and is similarly attached by a short rigid pedicel; but its form is usually much less distinctly spherical, undergoing slow irregular changes. The body is invested in a cuticle and the endosarc is usually full of reddish brown granules. The nucleus is spherical, like the solitary contractile vesicle which in one specimen was found to contract at intervals of from 34″ to 44″. The tentacles, which are the distinguishing feature, are much thicker than in an ordinary Podophrya, rounded, obtuse, and quite undilated at the summit, a little below which a circular constriction is seen. No other trace of organization is to be made out in them when the animal is quiescent. After slightly pressing the cover glass the tentacles may be seen to wave slowly about as if in search of prey; and this is accompanied by changes in length. On crushing the Cyclops and thus bringing to bear the stimulus of a large supply of food, the tentacles at once become very active. The part beyond the constriction expands into an open funnel: the rest shows numerous equidistant circular wrinkles, and actively lengthens and shortens. At the same time a canal is seen up the middle of the tentacle opening into the funnel; and food particles may be seen to pass along this into the body. The cuticle seems to form a fine investment to outside the tentacles. The number of these is from 6 to 14, scattered over the distal surface of the animal or sometimes apparently gathered in two groups.

These characters would almost seem to warrant the creation of a new genus, which, however, is best left to those who have a more critical knowledge of the Protozoa. For the present it may bear the name of *Podophrya ? infundibulifera*; the character of the tentacles giving the specific distinction thus:—

Podophrya infundibulifera n. sp.—P. tentaculis quiescentibus crassis ad apicem rotundatis, sub apice leviter constrictis, vel vivaciter elongatis et retractis rugis circulis dense striatis apice infundibulatis et pabulum aspirantibus.

Mr. C. Bailey, F.L.S., exhibited specimens of Ophioglossum Ambiguum from Barmouth, collected there by himself in July, 1879. This variety of the common O. vulgatum has been hitherto found only in the extreme North and South-West of the British Islands, viz.: in the Orkneys and Scilly Isles. Hence the discovery of an intermediate locality is of much interest. The exact locality was damp sandy ground, close to the sea, near Dyffryn Railway Station, between Harlech and Barmouth.

Mr. Bailey also exhibited a very complete and beautifully mounted collection of Scandinavian Hieracia, or Hawkweeds.

General Meeting, December 2nd, 1879.

R. Angus Smith, Ph.D., F.R.S., &c., in the Chair.

Professor Alfred Milnes Marshall, M.A., Fellow of St. John's College, Cambridge, Professor of Zoology, Owens College, was elected an Ordinary Member of the Society.

The following Minute from the Owens College was read:

"At a meeting of the Council held at the College on Friday the 21st November, 1879,

"It was Resolved:

"That the Treasurer be requested to inform Dr. Joule that the Council is deeply impressed with the importance of proceeding with the erection of the buildings necessary for the due accommodation of the Natural History Collections, so soon as an appeal can be made to the public for the necessary funds with a reasonable prospect of success."

On the motion of Professor Reynolds, seconded by Dr. Balfour Stuart, it was resolved, That the Minute from the Owens College be published in the Proceedings.

Ordinary Meeting, December 2nd, 1879.

R. Angus Smith, Ph.D., F.R.S., &c., in the Chair.

Professor Schorlemmer, F.R.S., translated a passage from a drama by Christian Gryphius, entitled: "Der deutschen Sprache unterschiedne Alter und nach und nach zunehout that the German language would be held in much higher estimation and more studied by foreigners if men of science would write in their own language, as it was now done in England. Every one wished to learn English since a *Boyle, Hook, Spraat, Backon, Brown, Simson, Plot, Blunt, Gildrey, Sibbald,* and many more ingenious men investigated and elucidated the innermost secrets of Nature in their own language.

"On a Peculiar Feature in the Water of the Well in, Carisbrooke Castle, Isle of Weight," by HARRY GRIMSHAW, F.C.S.

The sample of the above water was taken by me on April 19th, 1878, but was never opened or interfered with in any way until the following September. The water when taken was very bright and clear and free from sediment of any description. It was totally devoid of odour and was of a fresh and sparkling taste. The local features of the well from which the water derives its origin are as follows:—Carisbrooke Castle stands on a small isolated chalk hill, 239 feet above the level of the sea. The well is under cover in the "well house," and according to Jenkinson is 240 feet in depth. It is perfectly free, even at the surface of the water, from carbonic acid gas, or in fact of more than traces of any other gas than atmospheric air, as a candle floating on the water burns freely.

The bottle containing the water was opened on the 12th of September, and on doing so a very strong odour of sulphuretted hydrogen was perceived, and on testing with lead paper an equally strong reaction for that gas was obtained. There was also apparent a slight sediment of a white colour not originally seen in the water. The reaction to litmus was perfectly neutral. The analytical data obtained, which were all that was possible from the quantity of water at command, were as follows:

Total Solid Matter.	.42·00	grains per gallon	(consisting of)
Mineral Matter	......22·40	„	and
Volatile Matter	......19·60	„	
Total Hardness	......12·30	„	
Magnesia Hardness..	1·70	„	
Chlorine..............	4·50	„	

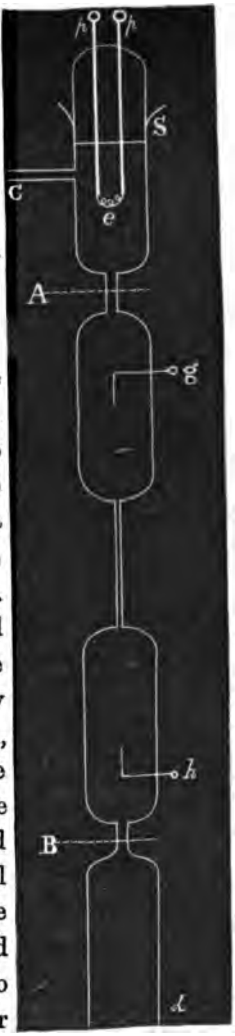
The residue on heating blackened very much, and emitted a very strong unpleasant odour like burning animal matter.

The peculiarity of this water is of course the production of sulphuretted hydrogen, on standing for some time (in this case for five months) out of contact with the atmosphere. On leaving a small portion of the water in the bottle again corked up for some time the presence of sulphuretted hydrogen was not exhibited. This production of sulphuretted hydrogen proceeds undoubtedly from the reduction of the sulphates contained in the water by the excess of organic matter, and it is not unique in this instance, although it is not a fact of very common occurrence. I regret very much not having been able to bring back a sample large enough to admit of a determination of the albumenoid ammonia, and the nitrates and nitrites, as the quantity of these substances in a water of such a description would have been a very interesting item in the case, as touching on a point of great importance in pronouncing an opinion on the quality of a water. It is very possible and even probable that some chemists, given to judging for and against a water chiefly by indications of a single description, to the comparative neglect of many other analytical results, say by the amount of albumenoid ammonia, might actually fail to condemn a water such as the above; for many chemists appear to consider the ignition of the solid residue a comparatively unnecessary detail; and supposing even that the nitrates had been determined in this water, there are eminent chemists who have considered the presence of nitrates in deep well waters, in the chalk especially, as comparatively innoxious. The Carisbrooke well, as I have said, is in the chalky strata, and is 240 feet deep, and whether it contains nitrates or not, is, in my opinion, on the results above detailed, a most unfit water for potable purposes, and my reason for bringing the analysis of this water before the Society was to draw attention to the tendency often exhibited to draw the chief inferences of the quality of drinking waters from what may be called isolated reactions, without obtaining, or at all events without giving weight to, other indications, chemical and physical, which they exhibit.

"Note on the Identity of the Spectra obtained from the different Allotropic Forms of Carbon," by Arthur Schuster, Ph.D., F.R.S., and H. E. Roscoe, LL.D., F.R.S.

Spectrum analysis serves as our most delicate test of the chemical constituents of a substance. Hence it appeared not uninteresting carefully to examine the nature of the spectra obtained by the combustion of natural graphite and of diamond in a vacuum of pure oxygen, and to compare the spectra thus obtained with the well known spectrum of carbonic oxide obtained from charcoal. The preparation of such an oxygen-vacuum which shall yield an oxygen spectrum exhibiting no other lines than those of oxygen is a matter of considerable difficulty. The slightest trace of any impurity containing carbon produces the spectrum of carbonic oxide. For this reason the use of caoutchouc tubing and of greased stopcocks must altogether be avoided, and thus the experimental difficulties are considerably enhanced.

In order to obtain a spectrum of pure oxygen entirely free from the lines of carbonic oxide, a necessary preliminary condition of our experiment, the following arrangement was



made. Fig. 1 exhibits the form of the tube employed. The part from A to B consists of an ordinary Plücker's tube. At the lower end of this a piece of hard glass tubing (*a*) was sealed. Before the experiment, the requisite quantity of permanganate of potash or oxide of mercury was brought into this to serve as the source of the oxygen, and then the tube was sealed at the lower end.

The other end of the Plücker's tube was closed by a ground glass stopper (S), through the sides of which two stout platinum wires (*pp*) were fused, and these were joined together within the tube by a spiral of fine platinum wire (*e*), into which the graphite or the diamond was placed. To prevent leakage between the ground sides of the stopper and those of the tube a drop of mercury, rendered less fluid by the immersion in it of a bit of tin foil, was introduced into the joint. The tube was placed in connection with the air-pump by means of a side tube sealed on at (C). For this purpose a Sprengel pump was used, to which the side-tube was hermetically sealed. In this way and in this way only was it found possible to obtain a pure spectrum of oxygen. After the connection with the pump had been made, the whole tube was exhausted, and then the substance contained in the hard glass tube was heated. The oxygen which is given off was then removed by the pump, the tube filled a second time with oxygen, this again removed, and this process repeated over and over again, until at last no other lines but those of oxygen are seen, when the spark from an induction coil passes between the electrodes (*g* and *h*).

When this stage had been reached, and when especially no trace of the carbonic oxide bands could be seen in the tube, the platinum spiral (*e*) containing either the diamond or the graphite was rendered incandescent by means of an electric current.

The spiral contained sometimes a piece of natural graphite, sometimes a Cape diamond, but the result was the same in the two cases. As soon as the platinum spiral had been sufficiently heated, a channelled space spectrum appeared in the capillary part of the tube. This channelled space spectrum was carefully compared with the spectrum of carbonic oxide obtained from charcoal and found to be identical with it. No band or line could be seen in the tubes thus prepared which was not also seen in a tube containing carbonic oxide. The spectrum which appears when a Leyden jar is introduced into the circuit is different, but here also we found that every line was due either to oxygen or to carbon. Two lines were seen in the green and greenish yellow which are not contained in any map of the spectrum of carbon or of oxygen, lines which had not been seen in a great many oxygen tubes prepared and examined by one of us. But it was found on further investigation that these are really oxygen lines, which only appeared at very high temperatures. The capillary portion of the tubes we used were much shorter than that in the ordinary Plücker's tubes, and this accounts for the temperature of the incandescent as being higher than usual. As one of the lines is near the unknown aurora line, its wave length was determined and found to be 5591, showing it to be decidedly less refrangible than the aurora line.

The experiment was repeated in four different tubes and many times in each tube; but whether graphite or diamond was employed, no line was seen which was not also obtained in a tube of the same dimensions containing carbonic oxide.

Captain ABNEY, R.E., F.R.S., exhibited his photographs of the ultra-red portions of the Solar Spectrum, and first of all showed that the light transmitted by ordinary bromide of silver was of an orange tint, showing absorption in the lowest end of the spectrum. He then went on to explain how he had tried to load the molecules comprising this bromide of silver by means of gum resins, and that he had thus been enabled to photograph slightly beyond the lowest limit of the visible spectrum. Further researches proved that bromide of silver could be prepared in two molecular states, one that already shown, and the other in which absorption takes place in the red as well as in the blue. This was found sensitive to every radiation. He pointed out that the blue form of the silver bromide could be converted into the red form by simple friction, and that after friction it was insensitive to the ultra red radiation.

Prof. ROSCOE here exhibited the different preparations of gold in minute division made by Faraday himself, some of which transmitted blue light and others red, showing that at all events two cases of molecular condition exist in the case of metallic gold.

Captain ABNEY then threw upon the screen photographs of the prismatic spectrum, in one of which the lowest limit of the prismatic spectrum was reached. He demonstrated this on the black board, by setting up as ordinates the wave lengths of the various portions of the photographs as obtained from the photographs of the diffraction spectrum.

He then exhibited various photographs of the ultra-red portion of the diffraction spectrum, extending from 7,600 to about 11,000. He stated that the photographs from which he was making his final map were taken on double the scale, with twice the amount of dispersion.

He then showed various prismatic spectra, exhibiting different states of atmospheric absorption, in one of which Piazzi Smyth's rain band was markedly visible.

After a short discussion, Captain Abney exhibited some photographs of the spectrum in natural colours.

Ordinary Meeting, December 16th, 1879.

J. P. Joule, LL.D., D.C.L., F.R.S., &c., President, in the Chair.

"On a New Form of Marine Rain Gauge," by W. J. Black, Esq. Communicated by J. B. Dancer, F.R.A.S.

This marine rain-gauge for the collection and estimation of rainfall at sea on board ship, is a *cylindrical vessel*, open at the top and with a conical bottom, as in a wine bottle, projecting upwards.

It is poised on an *upright pivot*, projecting into this from the floor of the square box, that encloses it, which is made large enough to allow of the swinging to and fro of the gauge all round.

The gauge is thus preserved in a *horizontal* position in all the rolling movements of a ship at sea, and it is further secured from spinning round on its axis *by pins* on opposite sides, moving in slots fixed on the corresponding sides of the box.

The gauge is formed of two parts, that fit into each other half way down, the *collector* on the top, and the *receiver* for the rain below, and each can be disconnected from the other at pleasure.

At the bottom of the collector is a *diaphragm* perforated with holes for the passage of the water into the receiver below, which will also check evaporation of collected rain, and prevent the upward splashing of the fluid by lurching of the ship.

From the bottom of the receiver is projected a right *cone*, hollow from below, which constitutes its *floor*, so that the rain collected in it will lie in the space between the cone and the enclosing cylinder.

The gauge can be easily taken off the pivot out of the box for the purpose of *emptying* its contents for measurement and replaced by the pins sliding down the slots on each side.

The box is square, and is broad enough and high enough to allow of the swinging of the gauge, and is provided with a *lid* on the open top, which secures the instrument from injury, and will close any further use of the gauge.

The *floor of the box* is perforated with holes to permit of the drainage of the rain falling down into it, and has handles at the sides for carriage, and a canvas cover. The contents of the receiver may be emptied, after disconnecting the collector on the top, and *measured* by the usual glass measure graduated to parts of inches of rainfall.

The box can be set down anywhere on the deck, as far as possible out of reach of any sea-spray, or secured on the *bridge of steamers* or the tops of sailing vessels, and left there for the collection of rain.

In the event of the admission of sea-spray with the rain, which is found uncommon in practice, it would be necessary to take the *specific gravity* of the mixture in the glass measure by the hydrometer, and thus find out the amount of each.

This gauge has now been experimented upon in several *ships of the Royal Navy* and Mercantile Marine, and found to answer perfectly, and the results as far as ascertained will be communicated.

The collection of rain at sea is found to be better carried on on board *steamers* than sailing vessels, &c., as there is in them less impediment by the sails, less rolling in motion, and more independence of the wind. There is very little apprehension of the *sea water* getting into it, as it can be placed on the bridge, or on the top of the deck house, quite free from spray, as is found in practice.

Abstract questions respecting the *difficulties* of correctly ascertaining the rainfall at sea by reason of winds, course of vessels, motion of ship, &c., may only be met by the answer that all rain-gauges at sea must be placed under like condition everywhere and at any time on board ship.

Some general idea of *rain on the ocean* could only be gathered after at least 10 years' continuous observations, as the items of interest are few and far between, sometimes many degrees of latitude and longitude apart.

It is found there is much *less rain* falling on the ocean than is generally supposed, and that the quantity of *fine weather* much exceeds that of stormy weather or wet weather in long voyages.

The greatest quantity of rain falls along the *equatorial regions*, the next in the tropical regions, while the intra and extra tropical regions are unexpectedly much drier than supposed. It will also be found there is much less rain falling at *sea than on land*, which is naturally as it should be, the warm ocean thus becoming the generator of the vapours that finally get condensed on the cold mountains of the land and descend to moisten the thirsty plains below.

Previous observations of rainfall at sea have been only taken by counting wet days, hours, and minutes, as may be seen in the meteorological returns of the Novara round the world in 1857–59.

The following tables are statistical summaries of the results of the observations taken by rain gauges on board ship at sea for a period of about 5 years over the Atlantic, Indian, Chinese, and Australian seas, but not on the Pacific:—

MONTHLY SUMMARY.—RAINFALL AT SEA.

		Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.	Tot'ls	Total Days
North Hemisphere.	Foochoo Rain					3·31	7·47							10·78	
	Wet Days					2	10							12	26
	Rate per diem	·734	·532	·136	·421	·659	1·20	·460	·410	·789	·397	·772	·134	·565	·135
	Wet Days	9	9	5	18	18	7	28	13	33	13	4	5	162	656
Equator	Rain Inches	6·61	4·80	·68	7·59	11·87	8·42	12·88	5·34	26·05	5·16	8·09	·67	91·15	
South Hemisphere.	Rain Inches	1·50	3·06	4·23	15·36	13·69	5·51	2·18	9·20	6·86	·40	2·88	1·47	66·33	
	Wet Days	8	11	14	25	25	25	13	35	14	1	6	2	182	787
	Rate per diem	·187	·278	·302	·614	·547	·220	·167	·262	·490	·400	·480	·735	·364	·092
	Figi Is. Rain	6·187	12·05	2·12	2·325						·40	·07	4·962	27·642	
	Wet Days	17	16	10	6						1	2	13	65	134
	Total Rain	14·30	19·97	7·08	25·27	28·87	21·40	15·06	14·54	32·9	5·56	5·97	6·80	195·90	
	Total Days	34	36	29	49	45	43	41	48	47	14	10	20	419	1605

SUMMARY.—REGIONAL.

Total General.....		195·902	418	·692		·175		44·25	
Regions.		Rain Inches	Rainy Days	Rate p. Diem.	Rate p. Deg.	Rate p. Tot. Day	Rate p. 100 Miles.	Rate p. Ann.	Latitude
Totals.............		91·15	162	·565	1·787	·137	·114	50·56	
North Hemisph.	Temperate—Wet	16·55	37	0·45	1·88	Total Days 658. Dry Days 496.	Miles, 80·087. Rate p. 1,000 Miles. Wet Days 2·02. Rate p. ann. 0·89.		40°—51°
	Dry	7·635	26	0·29	0·36				19°—39°
	Tropical—Wet ..	18·38	22	0·83	2·62				12°—18°
	Dry ..	6·86	12	0·57	1·71				8°—11°
	Equatorial......	41·72	65	0·64	6·00				1°— 7°
South Hemisph.	Tropical—Dry ..	18·708	47	0·41	1·55	Total Days 787. Dry Days 605.	Miles, 85·561. Rate p. 1,000 Miles. Wet Days 2·12. Rate p. ann. 0·84.		0°—12°
	Wet ..	23·94	44	0·54	2·98				13°—20°
	Temperate—Dry	16·97	59	0·28	0·84				21°—39°
		6·71	33	0·21	0·75				40°—48°
Totals		66·33	182	·364	1·381	·084	·077	30·76	
Total N. & S. Hems...		157·480	344	·464	1·590	·110	·095	39·77	
Total Foochow & Figi..		38·422	74	·671		·240		87·64	Days, 160

"On Screw Propulsion," Part III., by Rober
Assoc. I.N.A., Hon. Member of the Manchester L
Philosophical Society, Member of the Mathematic

On the Resistence of the Element (a) to Angu Translatory Motion.

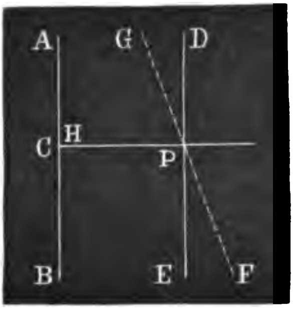
19. In the preliminary problem already di angular velocity (u) and the velocity (v) of transl the axis AB were assumed, without reference t by which these motions were produced and maintained.

In this problem the only object of investigati correct estimation of the velocity of the *elemen* normal direction when it was subject to a motion and translation measured by (u) and (v) respect

For the solution of this problem it was not necessary to enquire minutely into the cause by motions of rotation and translation were produc into the manner by which these motions are rel other during the time the cause of motion is in tion.

The application of the ordinary kinetic princ resolution and decomposition of velocities was qu for the purpose required in the problem, viz. t the normal velocity of the *element* (a).

It is otherwise, however, when the cause of th rotation and translation is the subject of consid this case the application is required of other principles than those which were found ad necessary in the solution of the preliminary pro



20. The *cause* producing rotation in the screw propeller is the force of the engine which is measured by a moment of force (M), acting at H, a unit of distance from the axis AB of the propeller. Or, in other words, the force (M), measured either in tons or in lbs. avoirdupois, acting at the point H in a direction perpendicular to the radius CP, must balance exactly the force of the engines which are applied to turn the screw round, and thereby to give motion to the vessel.

To a given constant moment of force (M), applied through the engines to give motion to the propeller in order to propel a given vessel, there belongs necessarily a definite uniform angular velocity (u) of the propeller, and, also, a definite uniform velocity (v) of the vessel.

The determination of these velocities, in terms of the moment (M), and the data supplied by the geometrical form of the propeller, is the chief object of the following investigation.

The case, then, to be considered here is, that in which the acceleration, produced by the constant application of the moment of force (M), has been entirely absorbed by the resistance of the water in which the vessel is moving.

21. In accordance with the usual and well known theory of plane areas striking fluids with given velocities, it follows that the normal pressure on the *element* (a) will vary in proportion to the square of its normal velocity.

The normal pressure, therefore, on the *element* (a) is, either

$$\mu\{ru\cos\nu - v\cos a\}^2 a \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(17)$$

or, $$\mu\{ru\cos\nu - (v - V)\cos a\}^2 a \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(18)$$

according as the propeller strikes the water when it is at rest, or when it is in motion with a velocity V in the direction of the vessel's motion.

The constant μ can be determined by exper
it is the normal pressure when, $v-\mathrm{V}=0$, and
upon the *element* (a).

22. The formulæ (17) and (18) give therefore
pressure on the *element* (a) in the two cases
sideration.

This resultant pressure, however, remains tc
posed, in the usual way, into three directions
direction of the axis AB, in the direction of the
in the direction of a line at right angles to the
respectively.

By the application of the principles of the de
of forces the following equations readily obtain.

Pressure parallel to AB = Thrust.

$$=\mu\{ru\cos.\nu-(v-\mathrm{V})\cos.\alpha\}$$

Pressure parallel to CP $=\mu\{ru\cos.\nu-(v-\mathrm{V})\cos.\alpha\}$

Pressure perp. to plane ACB $=\mu\{ru\cos.\nu-(v-\mathrm{V})\cos.\alpha\}$

23. It may be observed with respect to the
pressures that each of them acts at the point P.

1st. The amount of pressure indicated by f
corresponding to each element of the screw pc
be added together to balance the resistance to th
propeller.

If, therefore, this resistance, in accordance wi
theory, measured by $\mu^1 v^2$, then it follows that

$$\mu^1 v^2=\mu\Sigma\{rw\cos.v-(v-\mathrm{V})\cos.a\}^2 a\cos.a\ldots\ldots\ldots$$

where μ^1 is the resistance of vessel and screw w
with a velocity unity; and, Σ signifies the sum
tities

$$\{ru\cos.\nu-(v-\mathrm{V})\cos.a\}a\cos.a$$

for each point in the propeller blades.

2nd. The pressure indicated by formula (2
twist the axis of the propeller; and must be
by an equal pressure from another blade placed
site direction to CP.

3rd. The pressure indicated by formula (21),
ing to each element of the propeller blades, mu
together. Besides this force there is the skin

element (a); and, if this is considered to vary in proportion to the square of the velocity of the rubbing surface it will be measured by

$$\mu''\{r^2u^2\sin^2\nu + v^2\sin^2 a + 2ruv\cos\nu\cos a\}a$$

(see Art. 8) where μ'' is a constant to be determined by experiment. It follows then that

$$\left.\begin{array}{l} M = \mu\Sigma\{ru\cos.\nu - (v - V)\cos.a\}^2 a\cos.\nu \\ \qquad + \mu''\Sigma\{r^2u^2\sin^2\nu + 2ruv\cos\nu\cos a + v^2\sin^2 a\}a \end{array}\right\} \ldots\ldots(23)$$

where Σ has a similar meaning to that which is given in the first part of this article.

24. If, $V=nv$ equations (22) and (23) admit of the following forms readily obtained.

$$\left(\frac{w}{v}\right)^2 - 2A\left(\frac{w}{v}\right) + B = 0 \ldots\ldots\ldots\ldots\ldots\ldots(24)$$

Where, $A\Sigma(r^2\cos^2\nu\cos a)a = (1 - n)\Sigma(r\cos\nu\cos^2 a)a$

$$B\Sigma(r^2\cos^2\nu\cos a)a = (1 - n)^2\Sigma(\cos^3 a)a - \frac{\mu^1}{\mu}$$

And, $M = v^2\left\{C\left(\frac{w}{v}\right)^2 - 2D\left(\frac{w}{v}\right) + E\right\} \ldots\ldots\ldots\ldots\ldots\ldots(25)$

Where, $C = \Sigma\{\mu r^2\cos^3\nu + \mu'' r^2\sin^2\nu\}a$

$$D = \Sigma\{\mu(1 - n)r\cos^2\nu\cos a - \mu'' r\cos\nu\cos a\}a$$

$$E = \Sigma\{\mu(1 - n)^2\cos\nu\cos^2 a + \mu''\sin^2 a\}a$$

The quantities A, B, C, &c., are partly physical and partly geometrical, whose values can be readily calculated by integration over the surface of the propeller when μ, μ'', and n are given quantities.

By solving (24) as a quadratic

$$w = v\{A \pm \sqrt{A^2 - B}) \ldots\ldots\ldots\ldots\ldots\ldots(26)$$

Substitute this value in (25) and it becomes

$$M = v^2\{C(A \pm \sqrt{A^2 - B})^2 - 2D(A \pm \sqrt{A^2 - B}) + E\} \ldots\ldots(27)$$

25. If (n) is regarded as a constant quantity for the same ship and screw propeller, it follows from (26) that *the ratio of the speed of the ship to the angular velocity of the propeller is constant.*

The data furnished by the trials of the "Iris" (see Transactions of the Institution of Naval Architects for 1879) seem to confirm the truth of this inference.

The first series gives the following:—

Speed of ship $\quad 16{\cdot}577$

$$=\frac{82{\cdot}148}{15{\cdot}123}=5{\cdot}43$$

$$=\frac{65{\cdot}105}{12{\cdot}064}=5{\cdot}40$$

$$=\frac{43{\cdot}358}{8{\cdot}187}=5{\cdot}30$$

The remaining three series of experiments in the "Iris" afford the same confirmation of this inference as that given by the first series.

It follows also from (27) that *the resistance of ship and propeller is proportional to the force of the engines to turn the screw propeller.*

The truth of this inference is strongly confirmed by the following table given by Mr. Froude to show the manner in which the indicated horse power is applied "in the case of the Merkara; but the table, *mutatis mutandis*, will represent pretty nearly their relative values in other ships" (see Transactions of the Institution of Naval Architects 1876, page 175, fig. 7 plate XI.) :—

Speed of ship in knots per hour.	H.P. friction of unloaded engine.	H.P. friction of loaded engine.	H.P. air and feed pumps.	H.P. friction of screw.	H.P. augment of resistance.	H.P. net resistance.	H.P. indicated (Total).	H.P. friction of engine, or sum of A, B, C.	H.P. available to turn screw.	Values of $\left(\frac{G\text{-}K}{\text{speed } 2}\right)$
	A	B	C				G	K	G-K	D
3	84	6	10	6	20	34	160	100	60	6·6
4	100	10	15	10	30	65	230	125	105	6·6
5	117	18	20	15	45	95	310	155	155	6·2
6	160	30	30	22	60	133	435	220	215	6·0
7	180	45	45	30	90	190	580	270	310	6·3
8	225	85	60	40	110	255	775	370	405	6·3
9	265	140	90	50	150	340	1025	495	530	6·6
10	290	185	110	65	195	485	1330	585	745	7·4
11	310	260	160	76	264	680	1750	730	1020	8·4
12	335	325	180	90	400	990	2320	840	1480	10·3

The column D should be constant in accordance with the above inference. It is nearly so, and the slight deviation in the higher speeds may possibly arise from a slight increase in the friction of the engine, or, a slight error in the approximate estimation of the above causes of resistances.

Another inference of importance readily follows from formula (27) viz. *that the best propeller for one ship is not generally the best propeller for another ship.* The force of this inference is apparent from the circumstance that the form and dimensions of the best propeller depend upon the values of μ, μ', μ'', n, which are generally different for different ships.

26. Professor Osborne Reynolds, in his interesting communication to the I. of N. Architects, 1876, has suggested the existence, in the wake area of a ship, of a variable velocity which increases considerably, when accounted for, the difficulty of integrating the values of the forces in (19), (20), (21); and, he applies this variability in the wake velocity to the explanation of the propeller's tendency to cause vibrations in the ship. It is not, however, easy to see how this effect is produced by the cause assigned, inasmuch as the force which tends to twist the axis is a factor of cos.β, a quantity which vanishes in conoidal surfaces, particular forms of which are used as propellers in Her Majesty's navy.

A problem, however, of some interest and difficulty presents itself in formula (27), viz., required the form of propeller so as to make the coefficient of v^3 a minimum for a given value of engine power?

A somewhat similar problem to the above occupied the attention of the early writers on the calculus of variations. Newton and others sought the form of the lines of a vessel of minimum resistance; but the limited success, even of Newton's solution, to practical shipbuilding does not afford much encouragement in a similar attempt, by means of the same calculus, to solve the problem of the screw propeller.

"On the Anal Respiration of the Copepoda," by **Marcus M. Hartog**, M.A., B.Sc., F.L.S.

In a note on *Cyclops* read at the British Association I pointed out that its respiration was exclusively anal. I have now made out the same in *Canthocamptus* (fam. *Harpacticidæ*), and *Diaptomus* (fam. *Calanidæ*). In all three the mechanism is the same; at regular intervals, after the backward sway of the intestine, the anal valves open for an instant and then close, giving just time for a slight indraught of water after the opening, a slight expulsion at the close. The necessary pressure to confine the animal seems to interfere somewhat with these movements, sometimes stopping them if excessive; hence I refrain from noting with illusory exactness the intervals between each respiratory movement.

It is to be noticed that the rectum contains as a rule liquid only, the bolus of fæces remaining in it but a short time. By endosmose the liquid in the rectum will tend to be at the same condition of gaseous saturation as the body fluid around it, kept constantly agitated by the backwards and forwards sway of the stomach. During the short interval that the anus is open an approach to gaseous equilibrium with the external water takes place, even despite the very slight movement of the water (shown by the little change of place undergone by suspended indigo or carmine particles). In the absence of any other suitable respiratory apparatus, no one can hesitate as to the function of the action I have described.

In the *Nauplius* larvæ of *Cyclops* and *Diaptomus* the working is slightly different. The rectum is a subspherical muscular sac, which at regular intervals contracts so as to leave a linear cavity (along the long axis of the animal), and immediately dilates, sucking up the water from without.

An anal respiration, such as that of *Cyclops*, is found widely among *Crustacea*—even those which have well developed gills like *Astacus*, which is one of the highest forms. It has been demonstrated in *Phyllopoda* and *Cladocera*, and is here probably the exclusive mode in *Leptodora*, as shown by Weismann. That it is therefore primitive, and should be expected to occur in the primitive or at least very generalised group of the *Copepoda*, is an obvious deduction. Hence I anticipate that the homœomorphic *zoœa* larvae of the *Decapoda* will prove to have this same mode of respiration.

If there be any connection between Rotifers and *Nauplius*, it is easy to make out the origin of the arrangement in the latter. The ciliated funnels and lateral canals of the former can only be of service when there is a thin unchitinised anterior surface through which water can transude into the cœlom: by the extension of chitinisation over the whole surface these organs lose their function and abort, while the cloacal "contractile vesicle" takes on an inspiratory as well as an expiratory function, and becomes more or less confounded with the rectum, from which probably, even in Rotifers, it takes origin.

Here must be noticed the wide diffusion of anal respiration in aquatic Insect larvæ, (alternate inspiration and expiration by the pumping movements of the rectum). This would point to a common origin with Crustacea.

A list of the groups in which anal respiration is made out may be added.

- *Vermes:*
  - *Rotifera.*
  - *Gephyrea.*
  - *Oligochæto Limicola.*
- *Echinodermata.*
  - *Holothuroidia.*
- *Arthropoda.*
  - *Crustacea* (general).
  - *Insecta* (most aquatic larvæ).

General Meeting, December 30th, 1879.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. Thomas Ward, Brookfield House, Northwich, and Mr. John Bell Millar, B.E., Assistant Lecturer in Engineering, Owens College, were elected Ordinary Members of the Society.

Ordinary Meeting, January 13th, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"The Radiograph," by D. WINSTANLEY, F.R.A.S.

I have described already one of the several arrangements which I have devised for the automatic registration of the solar radiance.* The instrument in question places a lead pencil on a sheet of paper and writes down therewith when, and for how long, the sun may chance to shine, but it makes no record of the intensity of his rays. I will now ask your attention to the description of another and much more perfect apparatus, one which continuously records the intensity of thermal radiation to which it is exposed. This instrument I have called the "radiograph." It consists essentially as follows:— A differential thermometer of which the stem is circularly curved is mounted concentrically upon a wheel of brass in a groove cut with that object for its end. This wheel is supported with its plane in a perpendicular position by a knife edge of hardened steel which passes through its geometric centre and rests on agate planes. The tube of the thermometer is partly filled with mercury—preferably through half its curve—and for the reason given in my description of the simple sunshine recorder, to which I have alluded, a little sulphuric acid is introduced as well. If we now arrange it that the centre of gravity of the solid portion of the system here described shall be below the surface of the planes on which it turns (and the apparatus is provided with adjustments by means of which the point in question can be moved) it is clear that the arrangement may be made to swing in pendulous oscillations, notwithstanding the presence of the liquids it contains, for these remain substantially at rest whilst the tube which holds them does, in fact, slide over them (and with very little friction too) in swinging to and fro through arcs of the circles of which its parts are curves. Both bulbs of the thermometer are closed. It is obvious therefore that the tension of the air or gas which they contain will be uninfluenced by the barometric variations of the outer air, the temperature of which latter being experienced equally in each bulb will also leave the equilibrium of the apparatus undisturbed. When however one bulb is more heated than the other, the air contained therein will press more strongly on the heavy liquid piston in the tube and wheel the swinging portion of the system round until a fresh position of equilibrium is gained, and this will be (providing that the centre of gravity of the system has previously been made coincident with the point on which it turns) when the tension of the gases is equal in both bulbs. In fact, in so far as now described, the instrument is a differential thermometer, and is that alone, differing in this from Leslie's, that it is a solid and accessible portion of the thing which moves, and not the liquid it contains. When however one of the

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* Proceedings of Manchester Lit. and Phil. Soc., Nov. 18th, 1879.

the apparatus becomes a "radiometer" in the proper meaning of the term,* that is to say, a measurer of the thermal radiance to which it is exposed and the intensity of which it indicates by variations in the angular positions of a needle prolonged from one or other of the radii of the wheel.

It is only needful now to so arrange it that this needle shall make a tracing of its curves on a cylinder driven by clockwork at an even speed, and the "radiograph" is complete.

Concerning the actual instrument I use, its wheel is 7·3 inches in diameter, and the weight thereof a trifle more than two pounds. The other portions of the apparatus are of the same dimensional proportions as are indicated in the sketch. Of course some delicate method of recording has to be employed, and I have thus far used the smoked paper process so much adopted in the observatories of France. In this way the "radiograms" which illustrate this paper were obtained.

When using the instrument to record the radiance of the sun I have hitherto exposed it in a box of copper surmounted by a dome of glass into which the bulbs of the thermometer project. The line which joins them is in the plane of the meridian of the place and the black bulb to the north. The box itself is supported at an elevation of four feet or thereabouts upon a stand of wood, the legs of which are firmly embedded in the ground. The stand itself is located at the extremity of a garden which overlooks a valley and the sea. A small window in the box permits the movements of the train to be seen and the promptness with which the apparatus acts to be observed. If a cloud "no bigger than a man's hand," and "light as a feather" in its texture, floats before the sun, and occupies but three or four seconds in its transit, its presence, the duration of its passage, and the degree of thermal obscuration it effects are at once set down.

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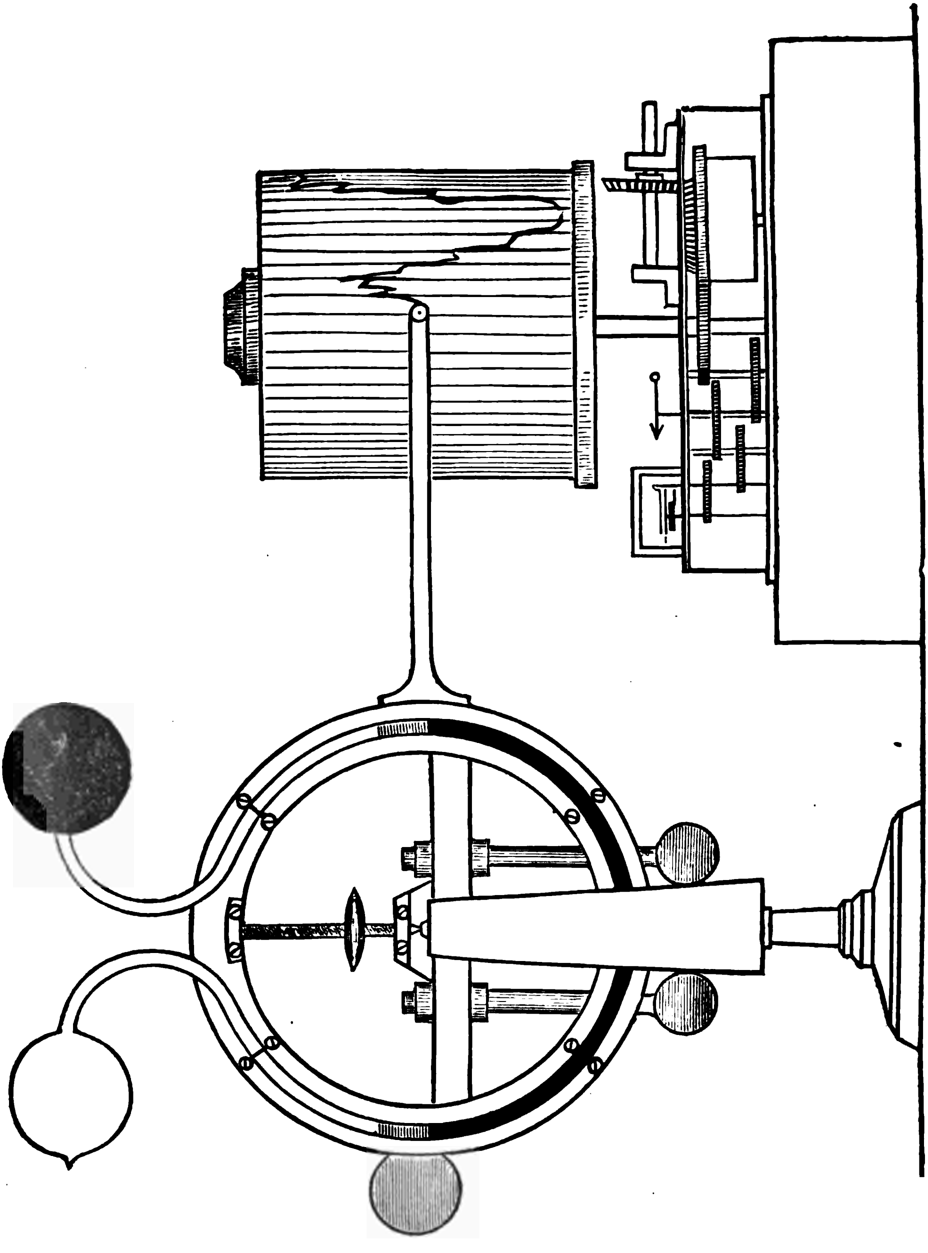
* The "radiometer" of Crookes should in its simple form have been called a "radioscope," as it merely makes visible the effects of radiance, but does not measure their amount.

The cylinder of the radiograph passes over a space of ·875 of an inch per hour, a somewhat open range, but, as will be seen on reference to the tracings, the needle often moves for some considerable distance in both directions along the same thin line, thereby showing a practical instantaniety of action under very ordinary thermal changes in the radiance from the sky. The influence of the sun's rays at daybreak is almost always shown, for some minutes at any rate, before the sun himself is seen, and occasionally it would seem even for hours before his time to rise.

It is not, however, now my purpose to dwell upon the interesting changes which take place in the intensity of the thermal radiance from the sky, my present object being to describe an instrument by means of which they may be recorded or observed. Doubtless in several of its details the "radiograph" may be improved, notably in the condition of its bulbs, and it would unquestionably be better if it computed for itself the areas included by its curves. This, I dare say, I shall presently enable it to do. Meanwhile, as a recorder of the duration and intensity of radiant heat the instrument, so far as I have seen, is the only one whose readings are uninfluenced by the temperature or the pressure of the air.



MICROSCOPICAL AND NATURAL HISTORY SECTION.

December 8th, 1879.

A. Brothers, F.R.A.S., in the Chair.

Mr. Herbert William Brockbank, of Manchester; Mr. John Aitken, of Urmston; Mr. Peter G. Cunliffe, of Handforth; and Mr. William Ward, of Burlington Street, Manchester, were elected Associates of the Section.

Mr. **Charles Bailey** exhibited a simple form of live box, which had been devised by Mr. Charles Botterill, of Liverpool, and which was likely to prove very useful to investigators who required to keep the objects examined under prolonged observation. It was suggested that a couple of small clips to keep the upper glass fixed would add to its convenience.

Mr. **H. A. Hurst** exhibited fruiting specimens of the rare Ambrosinia Barsii (Linn.), from Algiers, and read some notes thereon. Lindley classed it with the Lemnaceæ, but it is an undoubted Aroïd.

Mr. Hurst also read a paper "On the Protective Mimicry of Serpents exhibited by several of the Aroïdeæ."

January 19th, 1880.

Charles Bailey, F.L.S., President of the Section, in the Chair.

Mr. Edward Paul Quinn, of 137, Rydal Mount, Manchester, was elected an Associate of the Section.

Mr. **James Cosmo Melvill**, M.A., F.L.S., read an account of the occurrence of Silene Gallica (L.) and its sub-species in Jersey. He mentioned that it was the first recorded instance of the typical S. gallica (L) occurring within the limits usually assigned to the British Islands, if we except the recorded discovery of Dr. Bromfield in the Isle of Wight, many years ago, and never verified. He exhibited specimens of the five sub-species collected by himself at Gallows Hill, St. Helier's, Jersey, in June, 1879, and showed how that they were all certainly forms of one Protean plant, to which the term S. Gallica (L.) ought to be applied.

The forms were as follow :—

(I.) S. Gallica (L.) Stem erect; branches not spreading, but ascending; racemes somewhat dense; Lamina of petals obovate; roundish; large; entirely white; hairs of calyx appressed; fruit erect or patent.

(II.) S. Gallica-rosea, as the above, but petals rose-coloured. (S. Silvestris, Schott.)

(III.) S. Quinque-vulnera (L.) Petals white, with blood-red disk. The common form in Jersey.

(IV.) S. Anglica Quinque-vulnera. Petals coloured as in No. III., but of the form and size of No. V.; plant erect, ascending as in Nos. I.—III.

(V.) S. Anglica (L.) Stem somewhat flexuous; branches spreading; racemes not so dense as in S. Gallica; Lamina of the petals elliptical, somewhat jagged; very small; calyx with long hairs; fruit reflexed.

Mr. J. C. Melvill also exhibited specimens of Briza maxima (L.), a South European and Mediterranean grass, commonly called Large Quaking Grass, which was very rapidly spreading all over Jersey. He pointed out that it was both in Europe, the Canary Isles, and America, associated with two other grasses that have long been known as natives of Jersey, Bromus maximus, and Cynosurus echinatus (L.). The late Dr. M. M. Bull, of St. Helier's, informed him that he had first noticed it about the year 1872 in S. Saviour's parish, but that soon he expected it would overrun the island. It had not yet been noticed in the other Channel Islands.

"Bog Butter (*Butyrellite*), from Co. Galway, Ireland."

Mr. JOHN PLANT, F.G.S., exhibited a piece of a mineral resin familiarly known in the west of Ireland as Bog Butter. The lump weighed exactly 14oz. It came from a good depth in a bog in County Galway. A few years ago, when in that part of Ireland, he had been unsuccessful in meeting with a sample of this curious substance, although he was informed that it was not unfrequently met with by the turfcutters during each summer. He heard of its origin and of some of the uses to which it was said to be put by the poor people if they got any of it, from a farmer at Killkee, but he could hardly credit the statement that in hard times it was melted down and actually used as a dripping to the potatoes; he rather concluded that the greasing was limited to the axles of the potatoe cart. The Irish have a widespread belief that bog butter was hidden by the fairies in the bogs long ages ago; and it is affirmed that the butter is sometimes found in small wooden kegs in bogs along the coast. These kegs they say have been hastily buried by smugglers running a cargo of contraband, though when bog butter was declared an illegal article of trade in Ireland they are unable to say. Unfortunately, Mr. Plant was not shown a keg, or even the staver of a keg, but he was informed that specimens of veritable kegs of bog butter are to be seen in the Museum of the Royal Irish Academy and in the museums at Edinburgh. The fairy origin of the bog butter he thought might be ascribed to the active imagination of the Celtic brain, many of the inexplicable things in nature being readily put down to the good or evil doings of the indigenous fairies of Erin.

By the aid of scientific analysis the substance called bog butter can be shown to be a perfectly natural production arising from the decomposition of the vegetable matters forming the peat or bog, and to belong to the numerous family of mineral resins, or hydrocarbon compounds, of which Dana describes the composition of seventy species.*

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* Dana, System of Mineralogy, 5th edit., 1875, pp. 720–760.

Many of these are very well known under the names of marsh gas, petroleum, ozocerite, asphaltum, naphtha, paraffin, bitumen, amber, torbanite, coal, and its varieties.

Some of these singular minerals are obtained only from bogs and peat beds.

Some time ago Mr. Plant showed to the Section a quantity of one of these resinous minerals, which occurred under the bark of pine logs found in a moss at Handforth by Mr. P. G. Cunliffe. It proved to be known in Germany as Fichtelite, but had not before been known to occur in Great Britain. Afterwards it was found in pine logs in the peat on Lindow Common. A waxy, greasy, or butter-like character is distinctive of these bog products. The one now exhibited was described first by Brazier in 1825, and was analysed by Williamson in 1845, its composition being given as

Carbon..............................	73·78
Hydrogen	12·50
Oxygen	13·72

When fresh from the bog it is soft and like butter, but hardens in drying. The mass is dirty and bogstained on the outside, but inside pure white and free from impurities. It melts at 50° C., and becomes a yellow greasy resin, dissolves in alcohol or in ether, and then crystallizes in beautiful needles. When heated it gives off a peculiar odour like acroline. By saponification with potash it yields an acid which Brazier proves to have a composition similar to palmitic acid.

There is a mineral waxy resin called Guyaquillite, which is found in extensive deposits in the marshy plains near Guyaquil, in South America, which has a similar composition to bog butter.

Johnson gives it as—

Carbon..............................	76·67
Hydrogen	8·17
Oxygen	15·16

It has been proved that the slow decomposition or change in the vegetable peat or moss will produce elements of which these hydrocarbons are made.

PHYSICAL AND MATHEMATICAL SECTION.

January 20th, 1880.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

"On the Rainfall adjacent to the Sweetloves Reservoir, Sharples, for 1879," by Rev. THOS. MACKERETH, F.R.A.S., F.M.S.

The following rainfall observations for the past year have been made at a station which is 481 feet above the sea level, and on the southern slope of one of the several spurs of the Pennine chain that take a south-westerly direction. There are no less than seven rain gauges kept within a radius of three miles of my station. Two of them may be said to be in the town of Bolton itself, and of the other five two are on the slope on which my gauge is stationed, but from one to two miles west of me. The other three gauges are situated on a spur still further west, at the head of which stands Rivington Pike. But Belmont stands at the head of the spur on which I am situated. I have named the positions of these various gauges because I find no rule to ascertain any ratio of the relative amount of rain that may fall respectively into them. Sometimes a larger amount of rain falls in a gauge at a lower station than at a higher one, and at another time the reverse takes place. And this irregularity occurs too in guages respectively east or west of each other. Up to the end of 1877 a gauge was kept at Belmont at a height above the sea of 800 feet. It may be there yet for anything I know, but I can find no records of it after 1877. I have the records of that gauge for thirteen years, and in every instance they far exceed, with an almost constant ratio, the amount of rainfall registered in any of the other gauges to which I have referred. There is some reason for this regularity, as well as for the irregularity observed in the other gauges. It is an interesting enquiry to which I intend to devote myself, in the hope sooner or later to find a solution of the problem. I have good reason for thinking that the irregularity is due to the irregular surface of the country. When the late Mr. Vernon made his observations at Old Trafford, and mine were proceeding at Eccles, I could almost predict the amount of rain that would fall in a given month at his station, as he could at mine. The ground between the two stations was practically level, and though the gauges were between three and four miles apart, the rainfall at the two stations was practically the same.

The following table shows the results obtained from a rain guage with a 5in. receiver placed 22in. above the ground at this station :—

Days of Rainfall for each Quarter.	1879.	Fall in Inches.	Ditto in Quarterly Periods.
50	January	2·342	9·435
	February	4·407	
	March	2·686	
57	April	2·367	13·507
	May	2·628	
	June	8·512	
65	July	8·450	23·034
	August	10·630	
	September	3·954	
39	October	4·954	10·797
	November	1·604	
	December	4·239	
211		56·773	56·773

There can be no doubt that similar rainfall facts to those presented in the quarterly columns of the foregoing table have occurred throughout the country. That being so, it is clear that our crops, instead of being ripened by heat, were more than deluged in rain. August, which is usually the warmest month, experienced scarcely anything but a constant downpour of the heaviest rain. October is usually the wettest month of the year, but this year August has frightfully eclipsed it.

"A Curious Rainbow," by E. W. Binney, F.R.S., F.G.S.

On the 23rd of November last, at 12.45 p.m., I was on Douglas Head, in the Isle of Man. The sky was pretty free from cloud, except a slight cirrus overhead, the cirri running from north to south, and the sun shining. Looking southwards I noticed an inverted rainbow in the cloud at at an angle of 75° with the horizon, making an arc of about 30°. No other rainbow was visible at the time. I observed it for thirty minutes, when it gradually disappeared. The sea in the direction of the inverted rainbow would be about twelve miles distant. I did not observe that the colours were inverted as well as the bow, but a party who saw it stated that he distinctly noticed that they were. The accompanying rough sketch will show the form of the bow.

Ordinary Meeting, January 27th, 1880.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

"On Epidemic Cycles," by ARTHUR RANSOME, M.A., M.D.

It is somewhat remarkable that amongst the many observations made by the older writers upon the subject of epidemic diseases, no note appears to have been taken of their recurrence in regular cycles of years.

From the time of Hippocrates, and probably even before his epoch, much importance was ascribed to the rising and setting of certain stars, to the occurrence of comets, and even to eclipses of the sun and moon, and innumerable allusions are to be found to the influence of the seasons, and to variations in the atmospheric elements; but most of these circumstances are noticed rather with the view of accounting for their irregular appearance than with the object of showing that they observe regular periods.

The *καταστασις λοιμωδης* of certain years is indeed pointed out, and the rise and fall of certain epidemics at the several seasons is marked with marvellous accuracy, but it does not appear that any one observed their tendency to recur after the lapse of a certain, often very definite, term of years.

At first glance it appears strange that such an important phenomenon as this should have escaped the notice of such keen observers as many of the early physicians certainly were.

We can hardly doubt that such diseases as small-pox, measles, scarlet fever, and whooping-cough did actually appear in other countries with at least as much regularity as we shall presently find they did in Sweden. The plague also, if we may judge from the records given by Dr. Guy

(Public Health, p. 87) appears to have observed approximately a ten or fifteen years' period. But a little closer examination will probably account for the neglect to notice the recurrence of these events. The life of any one physician would be too short to admit of the observation of more than four or five such visitations, and he might well be excused if he hesitated to draw any conclusion from so small a basis.

Intercommunication between medical men was also more difficult than it has become now-a-days.

No numerical records of the causes of death were then kept for subsequent scrutiny, and it is probable that many of the less fatal forms of these diseases would pass through a nation without much notice.

Even when there is a registration of sickness as well as of deaths it does not always prove possible to mark the occurrence of an epidemic period. Quite recently, in 1868, Dr. Ballard, in an analysis of records of sickness kept in Islington for 12 years, was unable to find any indication of the cyclical recurrence of whooping cough, though he assigns a period of two years to measles, and four to scarlet fever.

It was, however, this disease, whooping cough, that first impressed upon my mind the fact that epidemic cycles do exist.

From the returns of sickness published every week by the Manchester and Salford Sanitary Association from the year 1860, it was evident that whooping cough tended to reappear biennially. Every second year the curve of this disease rose in the colder months of the year—in most cases epidemically. The rise and fall of the wave is almost perfectly regular. I have constructed a diagram that displays its course graphically, and it will be seen from this that whilst there are comparatively few cases in the winters of 1860, '62, '64, '66, '68, '70, '72, '74, '76, and '78, in the cold seasons of the intervening years there was always an increase in the number of persons affected. It is evident therefore that the complaint has a true two years' cycle.

I have noted the same regularity in returns of the disease made in St. Marylebone, London, by Dr. Whitmore.

In the same returns of disease there are indications of the observance of regular periods by other epidemic diseases, but the records as yet extend over too brief a time to show the fact with certainty.

The Registrar General's returns of deaths also present some evidence of a rhythmical succession of epidemics of such diseases as small-pox, scarlet fever, measles, and whooping cough; but the causes of death have only been given to the public since 1848.

Dr. Farr kindly sent me further returns for the five years 1838 to 1842, but, even with this addition, the area of observation is too short to permit of a satisfactory generalisation. I have, however, traced out diagrams giving the information to be gathered from these sources.

But in Mr. Simon's admirable papers relating to the History and Practice of Vaccination, presented to the General Board of Health in 1857, there is a diagram (which I have copied and placed before you) showing the small-pox death rates in the kingdom of Sweden for 100 years—from 1749 to 1849, inclusive.*

This diagram, though it is only used by Mr. Simon as an evidence of the protective influence of vaccination, shows distinctly the regularity of the small-pox cycle up to the year 1800, when vaccination was fully carried out.

A period of 100 years gives a fair basis for an indication. It therefore occurred to me to enquire from Dr. Farr from what source the Swedish records of small-pox were drawn, and whether similar tables could not be obtained for other diseases.

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*Although it is beside my present purpose, may I say incidentally that the chart in question proves to demonstration the wonderful protective power of thorough vaccination, and that I cannot understand how any one with this evidence before him can for a moment doubt its beneficent operation.

Dr. Farr, with his usual kindness, put me into communication with Dr. Berg of Stockholm, who then had charge of the Government department of the registration of deaths, and he at once sent me a list of the annual death-rates throughout Sweden since the year 1774, particularizing the mortality from scarlet fever, measles and whooping cough.

Dr. Berg accompanied the returns with a letter, which is appended to this paper, in which he pointed out the possibility of error in diagnosis having been made by the earlier Swedish ecclesiastical registrars in the country. In this caution, however, I think he does less than justice to those observers. The tables contain internal evidence of accuracy in the characteristic peculiarities of the course of each disease, and they bear ample witness to the fact of the regular succession of epidemics in distinct cyclical periods.

I have thrown these tables into the form of diagrams, and have supplemented them by similar charts drawn from the Registrar General's returns, and calculated so as to afford a means of comparing the two series of statistics.

The details of these charts will be more closely studied presently, but in the meantime it may be sufficient to point out that whooping cough has a cycle of about four years, that *small-pox*, before the introduction of vaccination, constantly re-appeared as an epidemic about every five or six years, that *measles* prevailed on an average about every seven years, and that *scarlet fever* had an extended period of fifteen to twenty years between the great visitations of this disease, with lesser undulations of three or four years.

I venture to bring these charts before the notice of this Society (1) because I believe they are unique in their kind, and (2) because they afford ground for a discussion of the probable causes of the remarkable regularity of epidemic cycles.

It is indeed unnecessary, with these charts before us, to enter into evidence to prove the existence of periodic times in epidemics, and we may perhaps be allowed at once to pass on to the modes in which such phenomena may be supposed to be brought about.

1. It is natural in the first instance to attempt to connect the epidemic cycle with some other coincident recurrent events. As Prof. Jevons says, in speaking of the principle of forced vibrations (Principles of Science, vol. ii., page 66), "Whenever we find two phenomena which do proceed, time after time, through changes of exactly the same period, there is much probability that they are connected."

I have already pointed out in several papers that there is undoubtedly some favouring influence exerted by seasons upon the different epidemics. Even in non-epidemic years there is an evident tendency for these complaints to rise at that season in which other affections of the organs most attacked by the special epidemic disease are most common.

Whooping cough and measles in the winter and spring, scarlet fever in the autumn, when sore throat is most common, continued fever also towards autumn, when derangement of the liver and bowels are frequent.

But it is impossible to discover any ordinary recurrent meteorological element that will account for the cycles of any one of these disorders. Atmospheric changes recur annually, but epidemics come every two, four, six, or more years.

There is indeed one interval that might possibly have some influence either directly or indirectly upon measles, small-pox, or scarlet fever that have an approximate five or six year period, and this is the well-known sun-spot period of astronomers.

I have obtained from different sources records of these occurrences since the year 1773, and have drawn out charts of the periods in a fashion similar to that adopted scale. An alteration in the extent of this scale is necessary in the year 1826, as the early observations are much less minute than those after this date, and include a smaller number of spots. Both series will suffice for the purpose of comparison with the mortality periods.

I must say at once, however, that I have been unable to discover any definite relation between them.

In the case of scarlet fever, there are truly some very curious coincidences between the rise and fall of the disease, along with the number of new spots appearing on the sun, notably in 1848, 1856, 1859, 1867, and 1870 (see diagram), but a little further scrutiny will show that at other times, as in 1843, 1850, 1854, and 1860, the extreme height of the disease corresponds with a low sun-spot period, and *vice versâ.*

2. In speaking of possible cosmical causes, it is right also to mention the theory of pandemic waves, originated by Dr. Lawson, Deputy Inspector General of Army Hospitals.

He believes that there exist world-wide influences affecting epidemics—influences that coincide in some way with the isoclinal magnetic lines; and from observations upon diseases in the army, he endeavours to show that there is a law by which "a series of morbific causes originate, or at all events become apparent, in the southern hemisphere, and are propagated from that to the northern with great regularity." "These waves occupy about two years in passing over a given station, the mortality from fevers and other epidemics increasing during their passage, and subsiding again as they move onwards." (Trans. Epidem. Soc., vol. ii., p. 95.) I do not now offer any opinion upon the value of this hypothesis, but I may point out that it can have very little bearing upon the question now before us. It regards rather the rate of progress and the geographical

only disease, whooping cough, that from our
two years' cycle is not included in Dr. Lawso
tions.

3. It has been suggested as an explanat
recurrence of epidemics after a certain perio
certain years of life are prone to the diseas
when these have been cleared away the diseas
prevail extensively until there were again pe
age to receive the poison.

This simple "age" theory will, however,
involve two important assumptions, in order to
cycles of several years' interval. 1. It will l
that the epidemic should in the first instance l
only attacking persons at the superior limit of
tibility. In the case of whooping cough at the
years, and in the case of the other complaints
or seven years of age; and 2, that it should
down to the lower ages, and so clear out of
persons capable of taking the disease that th
could not again appear until the most sensit
limit was again attained.

I need hardly say that neither of these ass
borne out by actual experience during an
epidemic disease. Children of all ages take th
both at the commencement and end of the epid

If therefore age alone were the determining
infectious complaint would be continuous, an
mittent in its ravages.

Owing to the constant occurrence of fresh
population, there must always be present in it
fit age to receive the virus; there is always food
it is able to flourish. The disease would only
if the birth-rate were also periodic in its chara

4. A fourth possible explanation of epidemic

perhaps be found in Dr. Farr's observations on the form of the epidemic wave and its probable cause.

In the year 1865 Dr. Farr showed that the curve traced by the course of the cattle plague was remarkably regular, and that other epidemics showed traces of a similar formation. It proceeded by continually decreasing increments from the first week to the last, and by noting the rate of diminution of the growing power of the disease in its first stages, Dr. Farr believed that it was quite possible to foretell its probable course and ultimate decline. He was actually able to do this for the rinderpest, and his forecast of the mode in which the disease would work itself out was proved to be singularly correct by the result. He accounted for this curve by the hypothesis that the poison of the disease was itself so diluted by successive transmissions through the bodies of its victims that it became gradually less and less able to propagate the complaint.

This hypothesis was supported by the history of most epidemics, by the shape of the curve it describes, by the fact of its greater fatality at the commencement of the outbreak, and by the observation that the longer the disease was absent from the community, the greater was its virulence when it did recur.

This theory would certainly account for most of these phenomena, though they are also susceptible of a simpler explanation, which I ventured to give in a paper "on epidemics studied by means of statistics of disease," which was read before the Oxford Meeting of the British Medical Association in 1868.

It is to be remarked also that no explanation is given by this hypothesis of the resurrection of the disease after a certain lapse of years. It would require the further assumption that after a period of latency the poisons gather strength enough again to attain an epidemic violence.

It has indeed been shown by Dr. Thiersch and by Dr.

Burdon Sanderson that the discharges of typhoid fever and cholera patients gain in virulence for a few days after their emission from the body, and Prof. Pettenkofer's 'ground' theory of the latter disease seems to demand the possession by disease germs of some such property as this; but no one has hitherto ventured to suggest that a seclusion of several years enhances the powers possessed by the materies morbi of these disorders.

5. Equally problematical is the suggestion that each epidemic is like a gigantic but disjointed organism, with a life history of the whole as well as of its individual parts.

This notion would be on a par with Mr. Butler's humorous idea that the "ovum when impregnate should be considered not as descended from its ancestors, but as being a continuation of the personality of every ovum in the chain of its ancestry." * * * each one of us being "actually the primordial cell which never died nor dies, but which has differentiated itself into the life of the world, all living beings whatever being one with it, and members one of another."

6. It would be more nearly in accordance with our knowledge of the ways of the lowest orders of beings if we were to suppose that after a certain lapse of time in which the disease germ was propagated by fission and by budding, it at length reached a period at which an act of conjugation was necessary for its continued existence, and that after this process the disease regained its former powers of offence and virulence.

7. I am inclined to think, however, that a much simpler explanation of the facts can be given than those hitherto brought forward, and this is, that a certain density of the population at susceptible ages is necessary before a disease can spread with the vigour of an epidemic.

Probably all the facts would be accounted for if we suppose that these disorders can only become epidemic when the promixity between susceptible persons becomes sufficiently close for the infection to pass freely from one to the other.

Exanthematous diseases rarely attack the same individual twice in his lifetime.

When, therefore, an epidemic has, by either a fatal or non-fatal attack, cleared away nearly all the susceptible persons in a population, mostly infants and children up to a certain age, then it must necessarily wait a certain number of years before the requisite nearness of susceptible individuals has been again secured.

There must in the interval be a gradual re-stocking of the nation with material fit for the epidemic to feed upon, and it can only spread when the requisite proximity is attained, when the meshes of the network of communication are sufficiently close for it to include all susceptible persons in one grand haul.

It will be found I think that this hypothesis is supported by the peculiarities of the different epidemic curves, and especially by the contrast between the courses followed by these diseases in England and Sweden respectively.

1. We may note the extreme height of the epidemic waves in the sparsely peopled country of Sweden, as compared with the comparatively small excess of mortality in epidemic years in the dense populations of England and Wales.

Thus the death-rates from measles and scarlet fever during an outbreak in Sweden are usually at least ten-fold those in ordinary years, and in the case of measles they sometimes rise to twenty-fold the number.

In England and Wales, on the other hand, scarlet fever never sinks below 7,000 deaths, nor rises above 24,000—a ratio of less than one fourth—and measles is still more certainly and equally present amongst us, the epidemic never marking double the number of deaths of ordinary years, and in some instances, as in 1863, only rising one-sixth more than usual.

In small-pox the difference is not so manifest, except that (in Sweden) in the 30 years 1772 to 1802, the waves of this disease are about ten times the height of the non-epidemic periods, a flood of disease that is only reached in England in the great outbreak of 1872.

2. We may also note the fact that the intervals between the epidemics are much shorter in England and Wales in the case of both scarlet fever and measles; and in Sweden, with an increased density of the population from 1860 to 1875, there is also apparent some shortening of the period of comparative immunity from scarlet fever.

It is indeed remarkable how very prevalent this disease has become of late years in Sweden. After 1861, it never sinks below a point that would have constituted an epidemic in former years, and in 1865 and in 1869 and 1870 it reaches nearly five times the height it had ever attained before 1830.

I have not seen any explanation of this extreme prevalence of the complaint in Sweden in recent times, though I have no doubt that it is in some way connected with the greater density of the population. The number of persons per square mile has just doubled in the course of 100 years. I would point out that an increase to so great an extent is not observable in the case of measles, which seems indeed to pursue the even tenour of its way in the last decade as in the first, only a slight increase being noticeable since the year 1820.

Is this because nearly every person takes measles, and only a portion of the population need contract scarlet fever?

3. We may remark that even in Sweden whooping cough and measles reappear as an epidemic after a longer interval when the previous outbreak had been a severe one. Thus, after the years 1793 and 1821, when measles reaches

its highest point, the cycle is extended to nine and eight years, and after prolonged and severe epidemics of whooping cough in 1790 and 1805 the interval is longer than usual.

4. When a small epidemic, or an attempt at an epidemic, occurs in the middle of a period, the interval is usually rather longer. This happens with measles in 1789 and in 1796, and in one case the gap between the highest points of the curve is eight and in the other nine years, instead of six or seven, as in other instances. In 1793 and 1817 there are also small waves of whooping cough postponing the usual outbreak of this disease from four or five to seven and nine years respectively.

It is interesting further to notice in this regard the inverse ratio that holds between the height of the epidemic and the lowness of the disease in the periods preceding and following it.

This is especially noticeable in the chart of whooping cough, in which the amount of disease constantly present gradually diminishes, and the epidemic waves rise higher and higher until the year 1806, and after this date the reverse process has been pursued. The disease, in fact, imitates the ocean in its spring and neap tides—though from a different cause—its ebb and flow following the same rule of a high tide with a low subsequent ebb.

Upon the hypothesis now before us all these facts would be readily explained by the varieties in the densities of the susceptible populations.

The degree of proximity of susceptible persons would be much more readily obtained in England in her crowded towns, and with her large number of births in proportion to acreage. In many parts of the country, in fact, it would probably be difficult for an epidemic to clear the ground so completely that in another year it would not again be filled up with a number of persons in sufficiently close proximity to one another to allow the disease to spread. In susceptible persons at any one time the sooner might the disease be expected to return, and after a time of comparative quiescence, so much the more virulent would it become on its return.

5. It seems probable that this theory will also account for several pecularities in the cycles of different diseases, and especially for the length of the interval between their chief epidemics.

Thus, *cæteris paribus*, the larger the range of the susceptible ages, and the longer is the epidemic period. When the disease has once thoroughly cleared off the susceptible persons at all ages, the requisite density for a great outbreak will be attained early or late according to the ages most prone to the complaint.

Thus whooping cough has a short cycle, and it is most fatal before the termination of the second year of life. It kills every ten years 37,000 males, and 43,000 females in the first two years of life, and only from 1,300 to 1,400 in the next two years of life.

Measles, on the other hand, takes its chief toll at and under six years of age. The numbers dying in the decade before the fifth year of life is completed are over 40,000 of each sex, but the disease still kills several thousands at ages from 5 to 10, whilst in the next 5 years period its number of victims sinks from thousands to hundreds. The actual numbers are:—from 5 to 10 years of age, 2,900 males, 3,200 females; from 10 to 15 years of age, 297 males, 358 females.

It is scarcely possible to draw any conclusions respecting small-pox, owing to the interference of vaccination with its natural course, but so far as the English returns of mortality go, they show that the age susceptibility is very similar to that of measles, and we know now that its cycle is not very different from that of this complaint.

Scarlet fever has the longest range of susceptible ages.

Although, like the other zymotic diseases, it kills the greatest number under five years of age, it retains its virulence to a much longer period than the other complaints. 28,000 males and 27,000 females are carried off by it from ages 5 to 10; from 10 to 15, 5,300 males and 6,000 females; but up to the age of 20 its number of fatal attacks in five years of life still reaches 1,500 and 1,600, and amongst females it continues to kill over 1,000 persons in the decade of life 25 to 35.

In relation to the lesser undulations of scarlet fever, we may also note the fact that this disease kills its largest number of victims between the ages 2 and 3.

I might at this point take my leave of the subject of epidemic cycles, but I should like also to bring before you one or two considerations suggested by the tables before us bearing upon the question of the possibility of abolishing altogether or mitigating the virulence of these complaints.

Sir Thomas Watson believes that he is not too sanguine in trusting that the abolition of zymotic disease may be looked for in the next generation, or at least by his grandchildren.*

And if it were possible to stamp out of the world every vestige of the contagion of any one disease, we might perhaps hope never to see it again amongst us. It would certainly need something like a universal deluge to accomplish this, but we may grant that after such a thorough annihilation it would take many cons of ages before the same disease could be evolved from the harmless bacterium or the common septic ferment.

I fear, however, that something far short of this would be the result of our interference, and the questions naturally arise, whether by keeping a disease at bay for a number of years, we may not be laying up a store of susceptible material for it to feed upon in time to come, and whether the epidemic when it does escape from its bonds may not prove more fearful than ever in its ravages.

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* Abolition of Zymotic Disease, Kegan, Paul & Co., p. 55.

Not only do these tables show the fact, but the whole history of epidemics proves that when a disease has been long absent from a country, on its return it is always both more virulent and more widely spread than when it is to some extent endemic amongst the people. This fact comes out very strikingly in the history of the small-pox, as given by Mr. Simon, and in outbreaks of measles in remote islands. Might we not then do serious harm by attempting to postpone the evil day of visitation?

This is a serious question for those who are now trying to arrest the progress of epidemic disease, and I fear that it would have to be answered in the affirmative if our energies were mainly devoted to isolation and disinfection whilst the other conditions are left intact, which are known to favour the propagation and vitality of these disorders. George Eliot is right in thinking that "you cannot fright the coming pest with border fortresses," and whilst filth, overcrowding, impure water supplies, and bad drainage are permitted to remain amongst us, it is highly probable that any temporary limitation of an epidemic will only serve to husband its strength, and give it more material to feed upon in the future.

But fortunately we are not necessarily impaled upon the horns of the dilemma of either being destroyed now or at some future period of our career.

In the case of the most serious of these maladies, scarlet fever, it is certainly not essential that every one should take it, and its power is probably greatly influenced by sanitary conditions.

I can hardly doubt that the great increase of this complaint of late years in Sweden is due to the circumstance that sanitary regulations have not kept pace with the growth of the population, and we must never forget that, in the words of Dr. Addison, "one blade of the destroying shears is forged at home, without it the other cannot do its work."

By improvements in the construction of dwellings, by thorough cleanliness, and other hygienic precautions, we may hope to place our crowded populations in at least as favourable a condition as the Swedes were 50 years ago, and epidemics may be once more made to observe their regular times and seasons.

We shall then moreover be armed with all the power that proverbially comes to those who are forewarned, and if we cannot arrest the progress of an epidemic we shall be able to take our measures to mitigate its ravages. We can then prepare our disinfecting armaments and our reception houses and hospitals for isolation, with some hope of a favourable result, and possibly at some future time it may be practicable to ensure a milder type of disease, or even, as in the case of small-pox, to substitute some allied and less fatal disease, which shall by anticipation rob the virus of the food upon which alone it can subsist.

Letter of Dr. Berg.

Dear Sir,

I am sorry that I cannot give you the wished for information such as you needed.

The forms for the registration of the causes of death have since 1749 often been changed, sometimes totally obstructed.

The ecclesiastical registrars in the country are not always able to make a correct diagnosis. Only for such diseases whose natures are generally known by the country people the information from older times can be supposed of some value.

There are to be distinguished three periods in the accompanying list of deaths.

The first from 1861 to 1874, where the numbers have real value.

The second from 1831 to 1860, when the registration of the causes of death which you ask was suspended. For a part of this time 1851–1860 I find in the reports of our Board of Health indicated epidemical exaggerations of *Scarlatina* in the years 1852 and 1857.

Measles ....1855, '56, and '57.
Hooping Cough..................'52, '53, '56, '57, '60.

The third before 1830, for which I have made extracts from the manuscript table in our archives, going back to 1774.

In these extracts the numbers for scarlatina seem to have no value, perhaps because this disease in former times may not have been generally and specifically known. In the numbers for hooping cough may probably be entered cases of capillary bronchitis. The measles is a disease generally known since a long time, and as I found the numbers of deaths indicating the periodical exacerbations, I have supposed them to be of value for you.

I have added the total of deaths from all causes, and the mean population for the years corresponding.

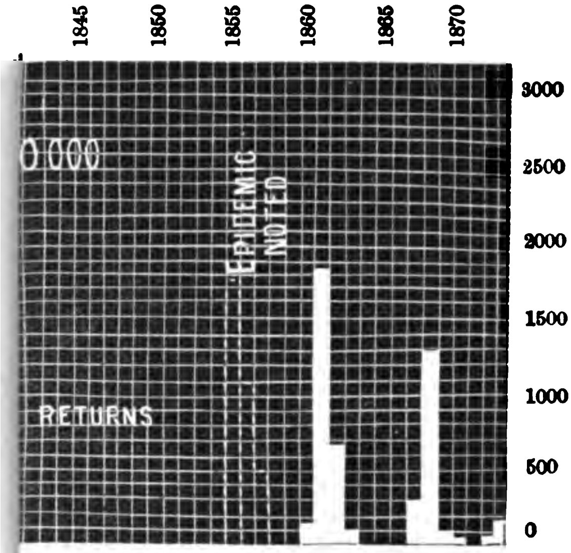
Most regretfully yours,

FRTH. BERG.

Stockholm, 17/11/75.

SWEDISH MORTALITY TABLES.

Year.	Registered Deaths from Scarlatina	Measles	Whooping Cough	Total Deaths	Population in the middle of the year	Rates per Million of Population. Scarlatina.	Measles.	Whooping Cough.
1874	2418	650	1309	87,760	4,319,766	555·0	150	365
1873	1244	293	1754	73,525	4,274,192	287·0	68	400
1872	1340	53	617	68,802	4,227,295	320·0	13	146
1871	2488	89	147	72,046	4,186,351	572·0	22	35
1870	4817	336	371	82,449	4,163,641	1150·0	85	89
1869	4396	5332	1262	92,775	4,165,910	1030·0	1300	313
1868	2199	1129	2414	87,807	4,184,381	525·0	270	575
1867	1903	126	768	82,072	4,178,179	456·0	31	83
1866	4063	165	468	82,666	4,137,409	968·0	39	114
1865	4448	182	602	79,216	4,092,101	1080·0	44	146
1864	2149	243	1148	81,937	4,046,313	590·0	61	275
1863	1354	2527	749	77,227	3,994,232	340·0	630	188
1862	1684	7407	1549	84,350	3,941,619	420·0	1830	395
1861	1453	507	1856	71,829	3,888,534	373·0	146	480
1830	129	524	2145	69,251	2,875,607	45·3	137	745
1829	221	5995	2084	82,719	2,854,960	77·3	2280	735
1828	739	3735	2117	75,860	2,837,254	260·0	1395	965
1827	389	446	2721	64,920	2,816,323	139·0	160	965
1826	356	396	3061	63,027	2,788,089	127·0	145	1070
1825	228	323	2271	56,465	2,749,065	84·0	119	800
1824	350	365	1513	56,256	2,707,954	130·0	135	510
1823	191	309	351	56,067	2,667,673	67·5	119	495
1822	243	650	2450	59,390	2,628,592	92·5	250	935
1821	176	6924	2990	66,416	2,597,780	67·5	2650	1195
1820		3129	1532	62,930	2,573,235		1190	600
1819		1556	1801	69,881	2,554,096		600	705
1818		470	1345	61,745	2,533,927		187	536
1817		477	2070	60,863	2,509,463		190	832
1816		426	1631	56,225	2,481,275		170	665
1815		1768	1174	57,829	2,454,804		715	485
1814		4203	1012	60,959	2,425,545		1750	410
1813		633	2127	66,266	2,412,114		265	878
1812		249	3458	73,095	2,402,130		102	1440
1811	63	301	2142	69,246	2,387,216	26·3	125	863
1810	90	320	958	75,607	2,379,963	38·0	135	403
1809	392	1574	628	95,532	2,400,458	158·0	655	263
1808	186	2436	563	82,311	2,426,781	77·5	1020	239
1807	65	500	2629	62,318	1,431,725	26·7	209	1043
1806	40	431	5046	65,728	2,428,069	16·5	180	2090
1805	38	400	1498	56,663	2,417,758	15·8	167	628
1804	72	367	440	59,584	2,399,973	30·0	152	184
1803	95	657	430	56,577	2,383,098	40·0	275	182
1802	198	2847	1057	56,035	2,365,193	80·5	1200	435
1801	73	633	3601	61,317	2,351,665	33·0	270	1550
1800	162	347	2320	73,928	2,352,148	70·0	150	1050
1799	137	453	1149	59,192	2,350,611	57·5	212	612
1798	246	511	1295	53,862	2,333,521	105·2	220	562
1797	360	588	3564	55,036	2,311,804	156·0	232	1550
1796	299	903	2744	56,474	2,290,965	130·0	390	1185
1795	264	562	878	63,619	2,274,064	120·0	250	392
1794	136	634	756	53,377	2,253,055	60·0	280	340
1793	168	3988	1160	54,376	2,225,381	71·0	1770	340
1792	167	2610	874	52,958	2,195,181	10·5	1190	528
1791	50	448	1139	55,946	2,168,476	23·0	219	478
1790	31	271	2924	63,598	2,169,999	14·3	128	1357
1789	59	618	2010	69,583	2,167,816	27·5	285	930
1788	29	248	2775	57,320	2,167,839	13·4	110	270
1787	53	283	1394	51,998	2,159,961	24·5	135	650
1786	61	2098	1447	55,951	2,152,941	28·4	980	653
1785	89	2411	1229	60,770	2,147,493	42·0	1120	573
1784	174	369	1239	63,792	2,144,392	82·0	175	576
1783	69	342	1752	60,213	2,142,278	32·3	160	845
1782	83	583	2524	58,247	2,136,949	39·2	273	1220
1781	138	438	1711	54,313	2,125,597	65·3	205	845
1780	174	296	1275	45,731	2,103,953	83·0	143	648
1779	255	514	1773	59,325	2,081,460	122·0	240	870
1778	330	2517	2416	55,028	2,065,222	162·0	1220	1209
1777	233	1551	1755	51,096	2,049,218	116·5	780	880
1776	217	231	2090	45,692	2,031,068	108 0	115	1050
1775	73	321	1852	49,949	2,009,328	36·5	160	[illegible]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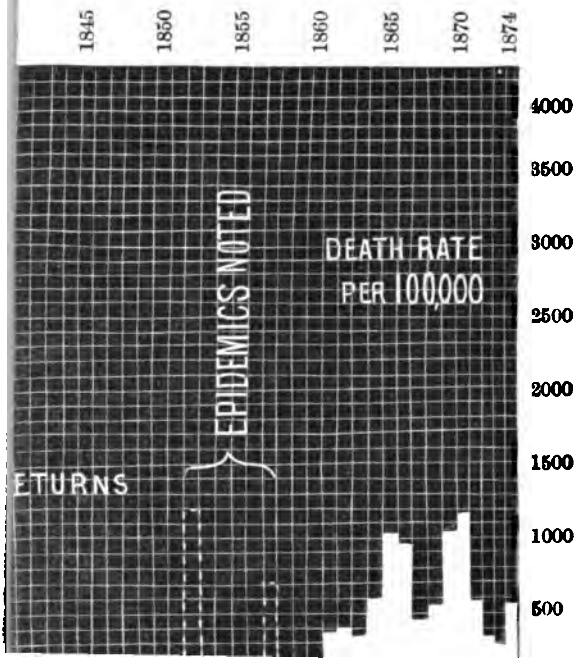
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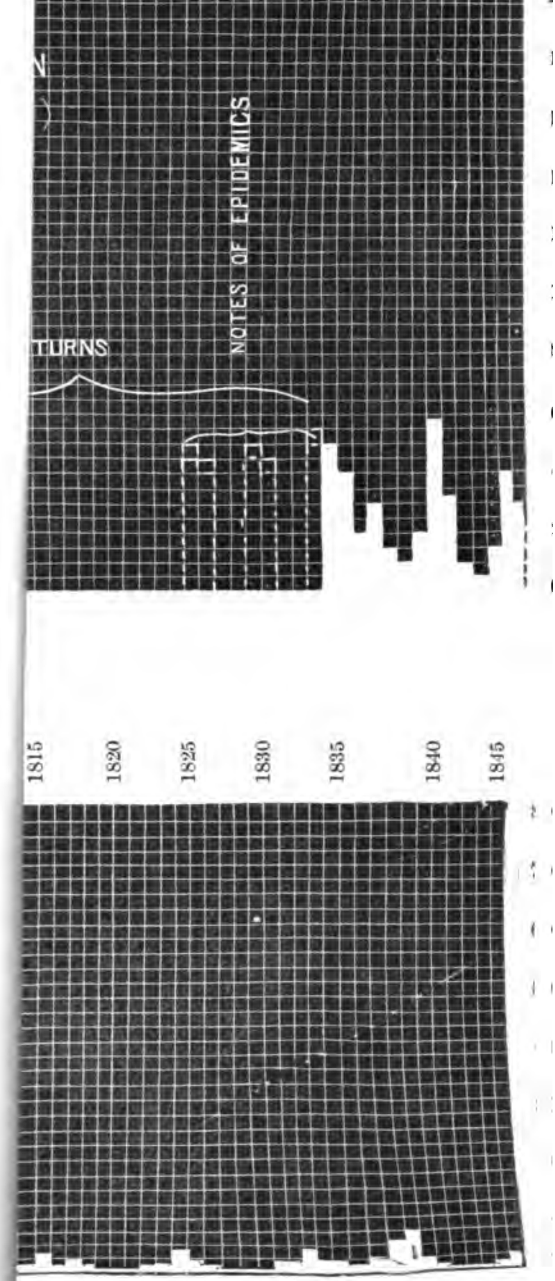
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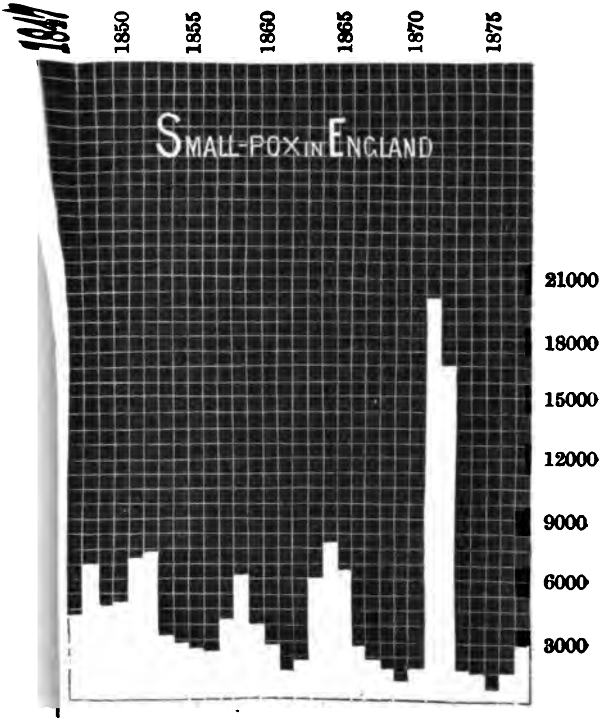
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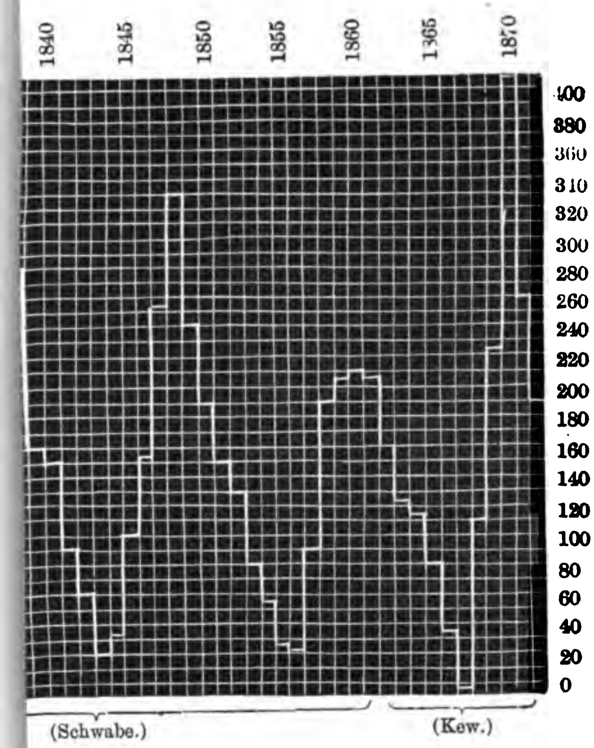
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Ordinary Meeting, February 10th, 1880.

E. W. BINNEY, F.R.S., F.G.S., Vice-President, in the Chair.

Professor ROSCOE, F.R.S., exhibited some specimens of phosphorescent surfaces prepared according to processes discovered by his late friend Mr. W. H. Balmain, of St. Helens. These preparations are now being largely introduced under the name of luminous paint. Mr. Balmain worked for many years in conjunction with M. Becquerel upon the phosphorescent compounds of the alkaline earths, and he has succeeded in producing a material which when exposed to diffused daylight, or direct sunlight, not only becomes strongly phosphorescent in the dark, but remains so for several hours after insolation. The proposal is now made to utilise this property for special illuminating purposes, and Dr. Roscoe showed various preparations made with Balmain's paint, kindly furnished him by Messrs. Ihlee and Horne, of Aldermanbury, London.

Professor W. C. WILLIAMSON, F.R.S., exhibited some specimens of Calamostachys Binneana, especially one recently discovered by John Aitken, Esq., in which the fruit displays both microspores and macrospores, the former occupying the upper and the latter the lower part of the same strobilus. This discovery equally separates the Calamostachys from the Equisetaceæ with which so many authors have associated it, and from the Gymnospermans Exogens of which Brongniart was inclined to regard it as a polleniferous spike. Professor Williamson has long advocated its association with Asterophyllites and Sphenophyllum, and ranked the whole with the Lycopodiaceæ. This interpretation seems to be now placed beyond doubt.

"Notes on a Bore through Triassic and Permian Strata, lately made at Openshaw," by E. W. BINNEY, V.P., F.R.S., &c.

For many years attention has been given by local geologists to the district lying between the Manchester coalfield and that of Ashton-under-Lyne and Oldham. The first authors that have treated on it were probably Messrs. Conybeare and Phillips, in their Outlines published in 1822. Mr. James Heywood, F.R.S., in a paper published in Vol. VI. (2nd series) of the Society's Memoirs, also noticed it. In a communication of my own, published in the 1st Vol. of the Transactions of the Manchester Geological Society, a horizontal section is given of the country between Manchester and Waterhouses, showing one great fault as then known. Afterwards, in Vol. XII. (2nd series) of our Memoirs, evidence is given of another fault at Medlock Vale, and lately in Part VI., Vol. XV., of the Transactions of the Manchester Geological Society, Messrs. Bradbury and Atherton have shown a third fault at Openshaw. As the district is thickly covered by drift deposits, and few natural sections are exposed, we have to wait till evidence is afforded by borings and sinkings. In several papers by me printed in the Memoirs, information has been given as it was met with, and as Mr. John Bradbury's boring is one of the most valuable, I wish to add it to my other communications on the subject, in order to make them more complete.

Mr. Bradbury's labours have shown the Permian sandstone, the one so well exposed at Vauxhall, in our city, to be of greater thickness than hitherto proved in the district, and as this deposit is a most formidable difficulty in sinking to the underlying coal seams, it is desirable that all information respecting it should be given to the public.

The bore was made near to the Ashton canal in Openshaw, and adjoining the boundary of that township with

Clayton. According to Messrs. Bradbury and Atherton, it was as follows :—

	Feet.
Drift Deposits	36
Trias (Pebble Beds)	46
Permian Marls, containing beds of limestone, one of which was 1ft. 4in. in thickness, and nodules of gypsum. In the lower parts of the marls and limestones were shells of *Schizodus obscurus*, *Gervillia antiqua*, &c.	200
Conglomerate	3
Permian Sandstone	752
Upper Coal Measures, containing 12 beds of Ardwick Limestone, one of which is 5ft. in thickness	263
	1300

The dip of the Permian strata was about 1 in 8, and that of the Upper Coal Measures 1 in 3 to the S.S.W.

The strata found resemble those at Medlock Vale, except that the Permian sandstone has increased from 420 to 752 feet in thickness. I have estimated that rock under Manchester at 400 feet, but its entire thickness has never to my knowledge been proved. In Chester Street, Chorlton-upon-Medlock, at the sugar works of Messrs. Fryor & Co., on the south side of the fault which runs from N.W. to S.E. between that place and the late Mr. Green's dye works, in Brook Street, the Permian marls and conglomerate bed increased to 260 feet in thickness, were found resting on upper coal measures containing the *Spirorbis* limestone similar to that at Ardwick without any trace of the Permian sandstone. Similar results have been found at the borings and sinking of the Seedley print works and the Patricroft colliery, and very lately Dr. Perrin informed me that the same occurred in a sinking at Plank Lane Colliery, near Leigh.

All the facts hitherto observed appear to show that the Permian sandstone is found of great thickness under the district lying between the Manchester coalfield and that of Ashton-under-Lyne and Oldham, while to the south of Manchester, under the Trias, it is replaced by the conglomerate of increased thickness. The former rock has probably never been deposited, nevertheless the fact of its general absence is of great importance to all parties who may sink for coal under the Trias.

At the present time the Permian strata of the N.W. of England and the S.W. of Scotland, so far as my knowledge extends, are represented in Lancashire in the following descending order :—

1. Upper Permian sandstone of Moat, Shawk, St. Bees, and Furness Abbey; absent in South Lancashire, without there is a representative of it in the Knowsley Quarry, near Prescot.

2. Magnesian marls with limestones and gypsum, containing *Schizodus obscurus*, *Gervillia antiqua*, and other characteristic fossils.

3. Conglomerate.

4. Permian sandstone of Vauxhall, Manchester.

5. Lower Permian sandstone of Whitehaven and Astley, by many English geologists taken to be uncomformable coal measures, but in Germany termed Lower Rothliegende.

The old Magnesian Limestone formation, as described by Professors Sedgwick and King, and my friend Mr. J. W. Kirkby, in the East and N.E. of England under the four first-named divisions, was pretty plain, although the line of demarcation between the Brotherton limestone and the Trias was not so easy to make out in all places. In the N.W. of England, and adjoining Scotland, the St. Bees sandstone, a rock of about 1,000 feet in thickness, cannot be traced distinctly passing upwards into the Trias, although doubtless it does somewhere betwixt Carlisle and the Solway; but in the valley of the Irk at Manchester the beds Nos. 4, 3, and 2 are seen lying on each other apparently passing into the overlying Trias, all the three rocks dipping at the same angle and in the same direction.

Near Manchester the occurrence of Permian fossils has enabled us to fix the position of the red sandstones and marls of the Trias and Permian beds, but after leaving Barrow Mouth, near Whitehaven, and traversing the country by Maryport, Carlisle, and Longtown to Canobie, as yet no fossil organic remains have been met with to help us, and we have to trust chiefly to superposition to separate the two formations. All who have investigated these formations know the difficulty of determining a Permian from a Triassic sandstone by external characters.

In some places in Lancashire the coal measures are covered by Triassic beds without the occurrence of the intermediate Permian beds, but near Manchester the latter are generally met with either as the upper deposits, the marls, and the conglomerate (Nos. 2 and 3) most frequently together, or with all the three beds of the series.

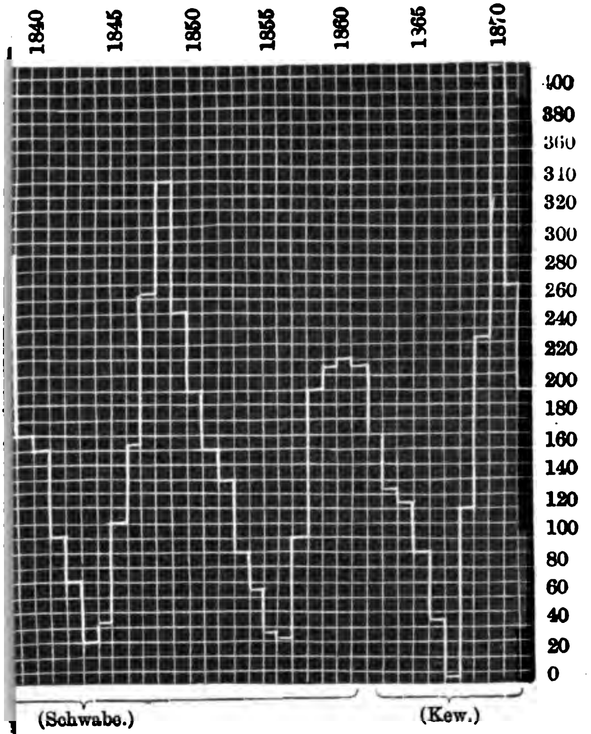
MICROSCOPICAL AND NATURAL HISTORY SECTION.

February 16th, 1880.

ALFRED BROTHERS F.R.A.S., in the Chair.

Professor Alfred Milnes Marshall, M.A., Owens College, was unanimously elected a Member, and Mr. Lionel E. Adams, of Victoria Park, Manchester, an Associate of the Section.

Mr. THOMAS ROGERS read a paper on the Land and Fresh-water Shells of Tasmania. He said that the total number of land shells in Tasmania, so far as at present determined, consisted of 115 species, comprised in five genera, the genus Helix alone being represented by 79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(Kew.)

opinion that the elasticity of the stone is due to the presence of mica is erroneous, for on close microscopic examination of a slice of the rock it will be seen that the flexibility depends upon the minute articulation where the sand grains interlock." "In my specimens," says Capt. Burton, "the stone abounds in light yellow mica, and when the friable material crumbles the two main component parts at once separate. Near Sao Thome das Letras there is a fine quarry of this elastic sandstone. In the deeper parts the strata become thin, and gradually pass into natural slabs of the finest quartzite, losing all elasticity. This flexible stone is not the matrix of the diamond and topaz, though sometimes associated with it."

A description of the Delhi flexible sandstone will be found in "The Records Geological Survey of India, vol. vii., p. 31, by H. B. Medlicott, A.M., F.G.S," of which the following is only an abstract:—

"The locality for the flexible sandstone is at Kaliana, near Dadri, about 60 miles due west from Delhi. The hill is 1,477 feet above sea level and 740 above the plain; it is a double-crested ridge from the Punjab.

"The projecting ribs on the double crest are formed of quartzite and strong ironstone bands, black hæmatite chiefly. These ferruginous bands are regularly interstratified with mica and hornblendic schist and an earthy cellular quartzite much quarried for millstones. All the strata are nearly vertical.

"The flexible sandstone is only found in patches in this millstone quartzite, there being no regular seam of it. The stone-cutters come upon it suddenly when cutting out slabs of the ordinary stone; often the rock in immediate contact with a nest of the flexible stone is highly indurated and quartzose. It is found both in the bedding and in the joints. The quarrymen's idea of the flexible stone is, that it is a mere local peculiarity of the sandstone rock caused by the percolation of rainwater and earth from the surface;" and Mr. Medlicott thinks that the water explanation is the correct one.

In his "Manual of Geology," Prof. Haughton gives his opinion upon the cause of the flexibility of the stone.

"A most remarkable circumstance sometimes occurs in the formation of sandstones which are not composed of pure particles of quartz, but a clay mixed with them, namely, that the particles of quartz mixed in this clay or paste are permitted a certain amount of motion." "If you take an ordinary sandstone, with a lens you can see the separate particles, and that each particle is touched on every side by a number of other rounded particles that hold them all in their places, so as to form of the whole a rigid rock." But in the Delhi stone—"you have a rock composed of particles of quartz which are not in contact with each other, but lie embedded in a paste of felspathic clay, which clay permits a certain amount of motion between the particles of the mass."

The specimen before the meeting came from the Kaliana quarry. When superficially examined it will be seen that it is not a schistoze rock, and hardly a particle of mica is to be found in it. It looks very like some sandstones of the Lancashire millstone grit. The grains of silica have a flattened and matted structure, the whole being porous and loosely held together—rather friable on the edges. It is freely interspersed with particles of a yellowish clay; it bends from its own weight, and when held up by one end freely oscillates. The bending is easy up to a certain point; it then becomes suddenly quite hard and rigid. Any further strain or a smart blow would then readily fracture it.

Mr. Alfred Morgan, Proc. Liv. Geo. Soc., vol. 3, p. 151, suggests that the true explanation of the flexibility of the rock might be, "That doubly metamorphosing conditions were concerned in its production, a solidifying process to give tenuity to the earthy paste, and a partial dissolution to remove the rigidity of its first solidification."

Mr. PLANT also exhibited some fine floccose infusorial earth in thick deposit at the gold diggings, Talbot, Victoria.

The following are some of the uses to which it can be applied :—

Uses to which Infusorial Earth is applied.

I. Article of food. Lapps mix it with bark of trees—in famine time Indians of Amazon eat certain white earths found on banks.

II. Manufacture of dynamite. The vehicle by which nitro-glycerine is capable of being transported. "After careful preparation, which consists in thorough removal of all organic matter by heat, &c., the earth is rolled and pressed and sifted, when it is ready for use. Fifty pounds of the earth is put into a flat wooden tank and covered with 150lbs. of nitro-glycerine, which is then thoroughly mixed with it. After half an hour has elapsed it is ready for removal to the cartridge moulds, in which parchment paper is used."

III. In agriculture.

IV. Floating bricks may be made of infusorial earth.

Forms a suitable covering for ice, beer cellars, fire-proof safes, steam boilers, powder magazines, &c.

In pottery infusorial earth has received several important applications, ex.: when fused with borate of lime an excellent glaze is produced.

It has been of service in the manufacture of sealing wax, soap, paper, ultramarine, &c.

Infusorial earth will take up *three* times its weight of nitro-glycerine without becoming more than damp to the touch.

Mr. Plant next showed a specimen of very fine clay occurring under a deposit of dark chocolate-coloured basalt, 50 feet deep, and on a gold-bearing gravel or ancient river course. The clay is filled with leaves or the impression of leaves of a gum tree (eucalyptus) like the living species. It is a meiocene deposit.

Mr. Marcus M. Hartog, B.Sc., F.L.S., exhibited, on account of a visitor, Mr. Chadwick, a new organism, different from a foraminifer, which he had found in Levant mud.

Ordinary Meeting, February 24th, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Mr. R. S. Dale and Mr. S. C. Trapp were appointed Auditors of the Treasurer's Accounts.

Dr. R. ANGUS SMITH, F.R.S., exhibited a diamond he had received from Mr. Hannay.

"On the Long Period Inequality in Rainfall," by BALFOUR STEWART, LL.D., F.R.S., Professor of Natural Philosophy at the Owens College, Manchester.

1. If it be true that there is a variation in the power of the sun depending on the state of his surface, this variation might naturally be expected to make itself apparent through a corresponding change in the rainfall of the earth, so that when the sun is most powerful there ought to be the greatest rainfall.

2. While the connexion indicated above is that which most readily occurs to the mind, yet the difficulty of ascertaining the facts of the case in a manner bearing the smallest approach to completeness is so great as to be at present insuperable.

There is *first of all* an intense reference to locality in rainfall, so that the rainfall at one place may differ greatly from that at another place in its near neighbourhood. *Again* there are probably, in addition to possible secular inequalities, very great oscillations in the yearly rainfall at any one place, or accidental variations, as we may term them in our ignorance of their cause.

Thirdly, it is in comparatively few places, and those places chosen not with the smallest reference to this particular problem, that we have anything like a trustworthy account of the rainfall throughout a considerable number of years.

Fourthly, we have no information of any importance with respect to the rainfall at sea.

3. Besides the formidable catalogue of difficulties now mentioned, we ought to bear in mind the following considerations. The convection currents of the earth are regulated by two things, one of which is constant, while the other may be variable. The constant element is the velocity of rotation of the earth on its axis, while the element of possible variability is the power of the sun. Hence it follows that if the sun be variable it will cause a variation in the direction as well as in the intensity of the earth's convection currents, on the principle which tells us that the resultant of two forces, one constant and the other variable, must vary both in magnitude and direction.

Now if it be true that we have a long period variation not merely of the intensity, but also of the distribution of the earth's convection currents, and if we bear in mind the intensely local reference in rainfall, it would be too much to expect that the rainfall inequality should exhibit the same years of maximum and minimum at all places. It is even conceivable that some places might exhibit a maximum, when others showed a minimum, while others again might exhibit a double instead of a single period.

4. It appears to me that if we bear in mind these considerations it will not answer to add together the rainfall of a few selected stations as they stand, with the view of determining by this means whether there be a long period inequality in the rainfall of the *whole Earth.* We are not yet in a position to reply experimentally to this question. It does not however follow that nothing can be done. Dr.

Meldrum and others appear to have achieved good preliminary work in the direction of indicating the existence of a rainfall inequality depending upon the state of the sun. Dr. Meldrum began by pointing out that in a good many places there is a greater rainfall during years of maximum than during years of minimum sun spots, and that this phenomenon repeats itself from one solar cycle to another. Again, Governor Rawson has pointed out the existence of certain localities where the rainfall inequality appears to be of a precisely opposite character, while Dr. Hunter has showed the practical importance of the investigation with reference to certain tropical stations. The subject has likewise been discussed by Smyth, Stone and others.

5. The question has arisen whether it might be possible to throw any light on this problem by the method of detecting unknown inequalities, proposed by Mr. Dodgson and myself (see Proceedings of the Royal Society, May 29, 1879). The essence of this method consists in a way by which we may numerically estimate the indications of an inequality. Let us suppose for instance that in ignorance of the diurnal range of temperature we try to find whether there be a temperature inequality of 24 hours or whether there be not rather one of 26 hours. We should begin by taking a large number of hourly readings of temperature and we should group these into two series, the one containing 24 numbers in each horizontal row, and the other 26. We should thus have 24 vertical columns from the one series and 26 from the other, and we should take the mean of each vertical column of each series as well as the mean of the whole.

Now it would speedily be found that an inequality was indicated by the 24 hourly series and none by that of 26 hours. For in the first series the mean of that vertical column representing observations at 5 a.m. would be greatly less than the mean of the whole, while the mean of that column representing observations at 2 p.m. would be much higher than the mean of the whole. On the other hand, in the 26 hourly series, provided it be sufficiently extensive, we should perceive no such differences. Thus in the 24 hourly series the differences of the means of the various vertical columns from the mean of the whole would be much greater than in the 26 hourly series, and the mean amount of these differences might be taken to form a numerical criterion of the presence or absence of an inequality.

6. This method therefore, applied to the subject in hand, might be expected to reveal the presence or absence of inequalities in rainfall, provided we have observations sufficient for the purpose. It is clear that the successful application of this method does not require a previous knowledge of the exact form of the inequality. Whether a maximum rainfall occurs at epochs of maximum, or at epochs of minimum sun spot frequency; whether there be only one rainfall maximum corresponding to the solar period, or two, or even three, is a matter of no consequence, as far as this method is concerned. All that is necessary is that the rainfall should always be similarly affected by similar states of the sun. Here however we must bear in mind that this method of detecting inequalities by summing up and averaging the departures from the mean caused by the inequality, likewise sums up and averages the accidental fluctuations. Now these accidental fluctuations are particularly large for rainfall, and it is therefore desirable to lessen their disturbing effect as much as possible. This can only be done by confining ourselves to long series of observations in which the accidental fluctuations may be supposed o counteract each other to a great extent, while the long period fluctuations will remain behind.

7. Through the kindness of Mr. Whipple, Director of the Kew Observatory, I have received copies of those catalogues

which was recently communicated to the Royal Society (January 8, 1880). Of these Paris, Padua, England, and Milan form the most extensive series, that of Paris embracing 161 years, Padua 154, England (Symons' table) 140, Milan 115. Mr. Whipple has likewise furnished materials by which the labour of applying the process in hand to these series will be much abridged, and he has kindly allowed me to make use of these. I will therefore apply the process to these four stations.

8. Let us begin by grouping the Paris yearly values into series of 8. We thus obtain the following final numbers expressed in centimetres :—51·4, 47·5, 45·7, 48·7, 51·1, 49·8, 46·5, 47·2, the mean being 48·5. From these we obtain the following series of differences :—

$$+2{\cdot}9-1{\cdot}0-2{\cdot}8+0{\cdot}2+2{\cdot}6+1{\cdot}3-2{\cdot}0-1{\cdot}3$$

In order to diminish the effect of accidental fluctuations, let us equalise this series of differences by taking the mean of each two. We thus obtain—

$$+0{\cdot}8+1{\cdot}0-1{\cdot}9-1{\cdot}3+1{\cdot}4+1{\cdot}9-0{\cdot}4-1{\cdot}7$$

If we now add these together, *without respect of sign*, and divide by their number (8), we obtain 1·3 as the mean departure from the mean of the whole, and bringing this into a proportional shape by dividing it by the mean rainfall (48·5), we obtain $\frac{1{\cdot}30}{48{\cdot}5}=2{\cdot}68$ *per cent.*

9. These explanations will enable the reader at once to perceive the principle of construction of the following table :—

	Proportional rainfall inequality, as exhibited by series of						
	8 years.	9 years.	10 years.	11 years.	12 years.	13 years.	14 years.
English rainfall, Symons' Catalogue.	2·63	2·14	1·55	1·79	3·15	1·69	2·57
Paris............	2·68	3·07	1·99	2·65	3·70	2·57	3·08
Padua	1·77	3·62	2·02	1·47	3·31	3·52	3·40
Milan	1·12	3·22	3·16	1·78	4·13	3·78	2·49

We ought to give the English, the Paris, and the Padua observations a somewhat higher weight than those of Milan, as the former embrace a longer period. This will be done sufficiently well by giving the first three sets weights of 3 each and the Milan set a weight of 2. If we perform this operation and then take the mean, we obtain as under:

	Proportional rainfall inequality, as exhibited by series of						
	8 years.	9 years.	10 years.	11 years.	12 years.	13 years.	14 years.
Mean of the 4 stations weighted as above.	2·15	3·00	2·09	1·94	3·52	2·81	2·92

A maximum corresponding to 9 years, and a still greater one, corresponding to 12 years, is thus exhibited, each of these being recorded at three stations out of four.

The proportional numbers indicated are not large, but it must be remembered that it is the mean difference for all the years that is given, and that the maximum and minimum rainfall will represent differences above and below the mean which will each be about double the numbers recorded above.

10. Regarding the rainfall values as representing the meteorological result of the sun's action, let us now compare these with declination-range values, which may be taken to represent the sun's magnetic effect. Professor Loomis has compiled (American Journal of Science and Arts, Second Series, Vol. L., page 153) what seems to be a very good table, exhibiting a set of yearly values of magnetic declination-range extending, with slight breaks, from 1777 to 1868.

Let us take this table and treat it precisely as we have treated the rainfall, except that it does not seem necessary to make any attempt at equalisation such as that made in article 8.

We thus obtain the following result:—

	Proportional declination-range inequality, as exhibited by series of						
	8 years.	9 years.	10 years.	11 years.	12 years.	13 years.	14 years.
Prague or reduced to Prague.	3·37	3·39	10·07	4·66	9·33	4·09	4·98

Here we have decided maxima corresponding to 10 and 12 years. The result is thus not unlike that which has been derived from rainfall observations where the maxima correspond to 9 and 12 years—indeed we could hardly expect a more perfect correspondence between the two, bearing in mind the limited amount of observations which we have for determining inequalities of long periods.

(*Note added on March 6th.*)

I take this opportunity of saying a few words on what I imagine to be the proper line of policy that should be pursued in this research.

(1) There are manifestly two stages in the investigation. *In the first place* we wish to ascertain whether there is any connexion between the state of the sun's surface (as revealed by spots) and the meteorology of the earth, and *in the second place* we wish to find the nature and laws of this connexion should it be proved to exist.

(2) If the various meteorological elements at the various stations of the earth are found to present the same periodic inequalities as those which characterise sun spots, this must be taken as decisive in favour of a connexion of some sort between the two, quite irrespective of the exact form of the inequalities. Nor will this evidence be invalidated if an inequality at one station should be different in form from that at another.

(3) Assuming the probability (from the evidence already brought forward) of such a connexion, the most natural hypothesis is that which supposes that the sun has inequalities which affect his radiating power. Hence it is of great importance (as proposed by Professor Stokes and others) to ascertain by judicious actinometrical experiments whether the heating effect of the sun's rays be in reality variable.

(4) In absence of actinometrical results we have grounds for believing that the magnetic activity of the sun is greatest at epochs of maximum sun spots, and it seems most natural that the meteorological activity of our luminary should be greatest when his magnetical activity is greatest.

From the reasoning of the paper to which this note is added we may conclude that there is no evidence which can be deduced from rainfall against this hypothesis.

(5) But while there is considerable preliminary evidence in favour of a variability in the heating power of the sun, and while this is constantly accumulating we must not deem it impossible that the sun affects the earth in some other way.

There is ground for supposing that the moon affects both the magnetism and meteorology of the earth in a way which we do not at present understand, and it is possible that the sun may have a similar influence.

Since writing the above I have learned that Mr. Baxendell made use of the method of mean departures described in this communication in one of a remarkable series of papers which he presented to this Society on March 8th, 1864. But I have no reason for supposing that he was aware of the peculiar characteristics of the method devised by Mr. Dodgson and myself in virtue of which we can with comparatively little trouble ascertain the exact periods of inequalities which are crowded very near together in the time-scale.

"On a form of representing the velocity at any point of an incompressible fluid under conservative forces," by R. F. GWYTHER, M.A.

1. The velocity at any point of a fluid may be represented in other forms than the usual velocity potential or the vector potential of Helmholtz.

The form $\sigma = \nabla\phi$ corresponds to the case when $S\sigma\delta\rho$ is a complete differential without a factor; let us imagine it to be made a complete differential by a factor that is to be of the form

$$\sigma = k\nabla\psi$$

or to be a combination of the two, thus—

$$\sigma = \nabla\phi + k\nabla\psi \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{I.}$$

2. First let us consider the circumstances accompanying the forms. If $S\sigma\delta\rho$ is integrable by a factor the condition is $S\sigma\nabla\sigma = 0$ or $S\sigma\rho = 0$.

lines of flow. A case satisfying the conditions of parallel cylindrical vortices, and circular vortex rings.

If $\sigma - \nabla\phi$ be integrable by a factor, the condition is $S(\sigma - \nabla\phi)\ \nabla(\sigma - \nabla\phi) = 0$ or $S(\sigma - \nabla\phi)\rho = 0$.

From this scalar equation ϕ could be found, and we may consider $\sigma = \nabla\phi + k\nabla\psi$ as a perfectly general form of expression for the velocity at any point.

3. The kinematical condition that σ must satisfy is the "equation of continuity," which if the fluid is incompressible takes the form $S\nabla\sigma = 0$ or

$$\nabla^2\phi + k\nabla^2\psi + S\,\nabla k\,\nabla\psi = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{II.}$$

and the angular velocity at any point in the fluid is given by

$$2\rho = V\,\nabla k\,\nabla\psi \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{III.}$$

The form taken by the equation of motion, when this expression is substituted for σ, will now be found; for

$$D_t\sigma = \nabla\left(V + \frac{p}{m}\right) \text{ may be written}$$

$$D_t(\nabla\phi + k\nabla\psi) = \nabla\left(V + \frac{p}{m}\right)$$

$$\text{or } D_t k\nabla\psi + D_t\nabla\phi + kD_t\nabla\psi = \nabla\left(V + \frac{p}{m}\right)$$

Now $D_t\nabla = \nabla.D_t - \Delta D_t$ where Δ acts only on the σ in D_t or $d_t - (S\sigma\nabla)$; therefore

$$\begin{aligned} D_t\nabla\phi + kD_t\nabla\psi &= \nabla.\,D_t\phi + k\nabla.\,D_t\psi + \Delta\ \{(S\sigma\nabla)\phi + k\,(S\sigma\nabla)\psi\} \\ &= \nabla.D_t\phi + k\nabla.D_t\psi + \tfrac{1}{2}\nabla\sigma^2 \end{aligned}$$

and finally the equation of motion is

$$D_t\nabla D_t\phi + D_t k.\nabla\psi + k\nabla.D_t\psi + \tfrac{1}{2}\nabla\sigma^2 = \nabla\left(V + \frac{p}{m}\right)$$

Now this equation is equivalent to three scalar equations and if we make in it the substitutions $D_t k = 0$ $D_t\psi = 0$, we reduce it to a single equation and solvable; for then

$$\nabla D_t\phi = \nabla\left(V + \frac{p}{m} - \frac{\sigma^2}{2}\right)$$

$$\text{or } D_t\phi = V + \frac{p}{m} - \frac{\sigma^2}{2} + \nabla^{-1}0$$

where $\nabla^{-1}0$ is independent of the position and may be included in the ϕ.

Putting for $D_t\phi$, $\dot{\phi} - (S\sigma\nabla)\phi$ and substituting for σ we get

$$\dot{\phi} - (\nabla\phi)^2 - kS\nabla\phi\nabla\psi = V + \frac{p}{m} - \frac{(\nabla\phi + k\nabla\psi)^2}{2}$$

$$\text{or} \quad 2\dot{\phi} - (\Delta\phi)^2 + k^2(\nabla\psi)^2 = 2V + \frac{2p}{m} \text{............IV.}$$

In support of the substitutions $D k = 0$ and $D_t\psi = 0$ I should state

1. That such surfaces can be found from the differential equation.

2. That only three scalar equations have been used in determining σ so as to satisfy the equation of motion.

3. That as the intersections of such surfaces, if they exist, are to move with the fluid, it is not unnatural to make the trial of the possibility, and a fourth we shall see later.

[It may not be out of place to notice that from the equation—

$$\dot{\sigma} - (S\sigma\nabla)\sigma = \nabla\left(V + \frac{p}{m}\right)$$

which may be written

$$\dot{\sigma} - 2\nu\sigma\rho = \Delta\left(V + \frac{p}{m} + \frac{\sigma^2}{2}\right)$$

we may by operating with $S\nabla$ get the equation

$$4\rho^2 - 2S\nabla\rho\sigma = \nabla^2 P$$

$$\text{where P stands for } V + \frac{p}{m} + \frac{\sigma^2}{2}$$

an equation I have never seen stated.]

4. In order to interpret as far as possible the expressions here introduced, we take first the last two conditions which express that the surfaces k and ψ move with the fluid so as always to contain the same fluid elements, and referring to the expression for the angular velocity we see that they intersect in the vortex lines.

It would be well to determine these surfaces more fully. We have as yet treated them as distinct, however the surfaces k and ψ coincide where vortex motion does not exist, for the $\mathrm{V}.\nabla k\nabla\psi = 0$ at all points, and the normals to the surfaces at all points are parallel. Whence the surfaces $k = \text{const.}$ and $\psi = \text{const.}$ coincide except at points where vortex motion exists. In order therefore to apply this method to the solution of rotational motion of a fluid, we should consider $k\nabla\psi$ an additive term, and take for k and ψ values such as to make the surfaces $k = \text{const.}$ and $\psi = \text{const.}$ move with the rotational fluid, and always intersect in vortex lines, while the term $\nabla\phi$ would be taken to satisfy the general irrotational motion.

5. To investigate the energy within a surface drawn within a fluid we get by continually using a modification of Green's Theorem and omitting Thomson's correction

$$\begin{aligned}
2\mathrm{T} = \int \sigma dv = \int \mathrm{S}\,(\Delta\phi + k\nabla\psi)\,\sigma dv &= \int \mathrm{S}\sigma\nabla\phi dv + \int \mathrm{S}k\sigma\nabla\psi dv \\
&= -\int \mathrm{S}\nabla\sigma\phi dv + \int \mathrm{S}\sigma\phi\nu a\mathrm{S} \\
&\quad -\int \mathrm{S}\nabla k\sigma\psi dv + \int \mathrm{S}k\psi\sigma r d\mathrm{S} \\
= -\int \mathrm{S}\nabla\sigma(\phi + k\psi)dv - \int \mathrm{S}\psi\nabla k\sigma dv &+ \int \mathrm{S}(\phi + k\psi)\sigma\nu d\mathrm{S} \\
&= -\int \mathrm{S}\psi\nabla k\sigma dv + \int \mathrm{S}(\phi + k\psi)\sigma\nu d\mathrm{S} \ldots\ldots\ldots\ldots\ldots\mathrm{V}.
\end{aligned}$$

where ν stands for the unit normal to the surface, and where the last term vanishes if there is no flow across the bounding surface.

We may then use the equation of continuity to give other forms to the volume integral. Thus

$$\begin{aligned}
\mathrm{S}\psi\nabla k\sigma &= \psi\mathrm{S}\nabla k(\nabla\phi + k\nabla\psi) \\
&= \psi\mathrm{S}(\nabla k\nabla\phi - k\nabla^2\phi - k^2\nabla^2\psi)
\end{aligned}$$

forms which will allow us to use Green's Theorem again.

From the rate of change of the circulation in a closed circuit moving with the fluid we get

$$\mathrm{D}_t\int \sigma d\tau = 0 \text{ or } \int \mathrm{D}_t\sigma d\tau + \int \sigma d.\mathrm{D}_t\tau = \int \mathrm{D}_t\sigma\delta\tau + \int \sigma\delta\sigma = 0.$$

The second is zero over every closed circuit. In order that the first may be so we must have $\mathrm{V}\nabla.\mathrm{D}_t\sigma = 0$, or referring to

$$\mathrm{V}\{\nabla\mathrm{D}_t k.\nabla\psi + \nabla k\nabla.\mathrm{D}_t\psi\} = 0 \ldots\ldots\ldots\ldots\ldots\mathrm{VI}.$$

a further justification for our assumption.

The simplest way of finding $D\rho$ is as follows. We have seen that

$$D_t\sigma = \dot{\sigma} - (S\sigma\nabla)\sigma = \dot{\sigma} - 2V\sigma\rho - \tfrac{1}{2}\nabla\sigma^2$$

operate upon this with $V\Delta$, then

$$V\nabla D_t\sigma = \dot{\rho} - 2V\nabla V\sigma\rho = \dot{\rho} - (S\sigma\nabla)\rho + (S\rho\nabla)\sigma$$
$$= D_t\rho + (S\rho\nabla)\sigma$$

But from the circulation $V\nabla D_t\sigma = 0$

$$\therefore D_t\rho = -(S\rho\nabla)\sigma \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots \text{VII.}$$

The value in terms of k and ψ is not so simple as to deserve notice.

The geometry of the motion is not easily explained owing to the fact that ϕ is not the third surface satisfying $D_t(x) = 0$, also k and ψ will not generally be independent, as the condition for irrotational motion is that they should touch at the points of intersection.

"Notes on some Quaternion Transformations," by R. F. Gwyther, M.A.

The following theorems are frequently required in physical problems, especially in the motion of fluids.

I.

If τ denote the vector of any point and ∇ Hamilton's operator, and if ρ and σ are any vector functions of τ, then

$$\nabla(\rho\sigma) = \nabla\rho\sigma - \rho\nabla\sigma + 2(S\rho\nabla)\sigma \ldots\ldots\ldots\ldots\ldots\ldots \text{I.}$$

More generally—If ρ and σ be any vectors depending on the scalars a, b, c, &c., and if α, β, γ, &c., be any vectors whatever; and if $\alpha d_a + \beta d_b + \gamma d_c + \&c. = D$, where d_a denotes differentiation with regard to a, &c., Then

$$D(\rho\sigma) = D\rho.\sigma - \rho D\sigma + 2(S\rho D)\sigma.$$

$$\text{For } \alpha d_a(\rho\sigma) = \alpha d_a\rho.\sigma + \alpha\rho.d_a\sigma$$
$$= \alpha d_a\rho.\sigma - \rho\alpha.d_a\sigma + 2(S\rho\alpha d_a)\sigma$$
$$\text{since } \alpha\rho + \rho\alpha = 2S\rho\alpha$$

If we now form the similar quantities for βd_b, &c., and add the respective sides of the equation thus formed

$$\therefore D(\rho\sigma) = D\rho.\sigma - \rho D\sigma + 2(S\rho D)\sigma \ldots\ldots\ldots\ldots\ldots \text{II.}$$

angular unit vectors. D in this case becomes identical with ∇, and the preceding form may be deduced.

The more general form is occasionally useful.

From I. we may by taking scalar and vector parts get

$$S\nabla(\rho\sigma) = S\nabla\rho\sigma - S\nabla\rho\sigma \ldots\ldots\ldots\ldots\ldots\ldots \text{III.}$$

$$V\nabla(\rho\sigma) = V\nabla\rho\sigma - V\rho\nabla\sigma + 2(S\rho\nabla)\sigma \ldots\ldots \text{IV.}$$

whence we may deduce by putting $\rho=\sigma$

$$V\nabla\sigma^2 = 2V\nabla\sigma\sigma + 2(S\sigma\nabla)\sigma$$

We may also deduce from (I) δ values for $\nabla(V\rho\sigma)$ and $\nabla(S\rho\sigma)$. To obtain forms more generally useful than the more general forms put $S\nabla\sigma=0$ $S\nabla\rho=0$ in

$$\nabla V(\rho\sigma) = \tfrac{1}{2}\nabla(\rho\sigma - \sigma\rho) = S.\nabla\rho\sigma - S.\rho\nabla\sigma + (S\rho\nabla)\sigma - (S\sigma\nabla)\rho$$
$$+ (S\nabla\rho)\sigma - \rho(S\nabla\rho)$$

and we get

$$S\nabla V\rho\sigma = S(\nabla\rho\sigma - \rho\nabla\sigma) \ldots\ldots\ldots\ldots\ldots\ldots \text{V.}$$

$$V\nabla V\rho\sigma = (S\sigma\nabla)\sigma - (S\sigma\nabla)\rho \ldots\ldots\ldots\ldots\ldots \text{VI.}$$

These forms simplify still further when in fluid motion σ is the velocity at any point and $V\nabla\sigma=2\rho$ gives the rotation at any point.

Again, Helmholtz's notation for the form of σ gives $\sigma=\nabla(\phi+\omega)$ where ϕ is a scalar and $S\nabla\omega=0$, and these formulæ are applicable in the reduction of the equations.

A slight adaptation of this method enables us to prove that if p and q are quaternion functions of τ, we should have

$$\nabla(pq) = \nabla pq + Kp\nabla q + 2(Sp\nabla)q$$

II.

These results are very useful in obtaining modified forms of Green's Theorem.

The general form of the theorem is

$$\int S\nabla\psi dv = \int S\psi\nu.ds.$$

where ψ is a single valued vector function in simply connected space, dv an element of volume, ds of its bounding surface, and ν the unit vector normal to ds drawn outwards.

Case I. Let $\psi = \nu(\rho\sigma)$.

Then by (V)

$$(\delta)\mathrm{S}\nabla\psi = \mathrm{S}\nabla\rho\sigma - \mathrm{S}\rho\nabla\sigma$$

and we get

$$\int \mathrm{S}\nabla\rho\sigma dv - \int \mathrm{S}\rho\nabla\sigma dv = \int \mathrm{SV}\rho\sigma.\nu d\mathrm{S} - \int \mathrm{S}\rho\sigma\nu d\mathrm{S} \ldots\ldots \mathrm{VII}.$$

Case II. Let $\psi = \phi\sigma$, where ϕ is a scalar.

$$\text{Then } \nabla\psi = \nabla.\phi.\sigma + \phi\nabla\sigma$$

$$\text{and } \int \mathrm{S}\nabla\phi\sigma.dv \int \mathrm{S}\nabla\sigma.\phi dv = \int \phi \mathrm{S}\sigma\nu d\mathrm{S}. \ldots\ldots\ldots\ldots \mathrm{VIII}.$$

Stokes' Theorem can be made by similar treatment to give varied forms, both more and less simple.

PHYSICAL AND MATHEMATICAL SECTION.

March 2nd, 1880.

E. W. Binney, F.R.S., F.G.S., President of the Section, in the Chair.

"Results of Observations of the double-period Variable Star *R Sagittæ*," by Joseph Baxendell, F.R.A.S.

During the last two years I have observed six of the principal minima of this interesting variable, and as I had not made a redetermination of its elements since the end of 1861, or only two years after the discovery of its variability, I have reduced and projected all my observations made since that time and have obtained the dates of 26 additional principal minima. The entire series from the discovery of the star's variability in 1859 to the end of 1879 is as follows:—

No.	Date.	Mag.	Days in Interval.	Number of Periods.
1	1859, Oct. 27	9·8		
2	1860, Jan. 7	9·6	72	1
3	„ May 31	9·7	145	2
4	„ Aug. 7	10·1	68	1
5	„ Oct. 19	10·3	73	1
6	„ Dec. 27	10·0	69	1
7	1861, May 1	10·0	141	2
8	„ July 25	9·7	69	1
9	„ Oct. 8	9·7	75	1
10	„ Dec. 17	10·0	70	1
11	1862, July 15	9·7	210	3
12	„ Sep. 19	9·2	66	1
13	1863, July 4	9·5	288	4
14	„ Sep. 11	9·7	69	1
15	„ Nov. 18	9·6	68	1
16	1864, June 19	10·1	214	3
17	„ Aug. 30	10·0	72	1
18	„ Nov. 6	9·4	68	1
19	1865, June 4	9·7	210	3
20	„ Aug. 15	9·7	72	1
21	„ Oct. 23	9·8	69	1
22	1866, May 25	9·7	214	3
23	„ Aug. 2	10·0	69	1
24	„ Oct. 12	9·8	71	1
25	„ Dec. 22	9·7	71	1
26	1867, Sep. 28	9·8	279	4
27	„ Dec. 5	9·9	68	1
28	1868, July 5	9·3	213	3
29	„ Sep. 12	9·3	69	1
30	1878, Aug. 19	9·1	3628	52
31	„ Oct. 30	9·6	72	1
32	1879, Jan. 10	10·1	72	1
33	„ Aug. 5	9·9	207	3
34	„ Oct. 15	10·0	71	1
35	„ Dec. 24	9·8	70	1

Dividing this series into two, the first containing the minima numbered 1 to 18, and the second those numbered from 19 to 35, and determining the mean epoch for each series, we obtain from the interval between the two epochs and the corresponding number of periods, the value of the mean period on the assumption that the period is uniform and only subject to accidental changes. This value is 70·154 days, and the mean epoch for the entire series of minima is

1866, July 25·815.

Calculating the times of minima from these elements and comparing the observed with the calculated times we obtain the following differences :—

No. 1.	– 8·44 days.	No. 19.	+ 4·11 days.
„ 2.	– 6·59 „	„ 20.	+ 5·96 „
„ 3.	– 1·90 „	„ 21.	+ 4·80 „
„ 4.	– 4·05 „	„ 22.	+ 8·34 „
„ 5.	– 1·20 „	„ 23.	+ 7·19 „
„ 6.	– 2·35 „	„ 24.	+ 8·03 „
„ 7.	– 1·66 „	„ 25.	+ 8·88 „
„ 8.	– 2·81 „	„ 26.	+ 7·26 „
„ 9.	+ 2·04 „	„ 27.	+ 5·11 „
„ 10.	+ 1·88 „	„ 28.	+ 7·65 „
„ 11.	+ 1·42 „	„ 29.	+ 6·49 „
„ 12.	– 2·74 „	„ 30.	– 13·51 „
„ 13.	+ 4·65 „	„ 31.	– 11·66 „
„ 14.	+ 3·50 „	„ 32.	– 9·82 „
„ 15.	+ 1·34 „	„ 33.	– 13·28 „
„ 16.	+ 4·88 „	„ 34.	– 12·43 „
„ 17.	+ 6·73 „	„ 35.	– 12·57 „
„ 18.	+ 4·57 „		

The distribution of the *plus* and *minus* errors shows at once that the period has not been uniform, but that it was longer in the earlier than in the later years. I therefore grouped the minima observed in the years 1859 to 1868 into four groups, and on forming the usual equations, and treating by the method of least squares, I obtained the following results :—

1859, Oct. 27, to 1861, Oct. 8, nine minima gave the mean period = 70·88 days.

1861, Oct. 8, to 1864, Aug. 30, nine minima = 70·46.

1864. Aug. 30, to 1866, Dec. 22, nine minima = 70.44.

1866, Dec. 22, to 1868, Sep. 12, five minima = 69.96.

The interval from 1868, Sep. 12, to 1878, Aug. 19, was 3628 days, and comprised 52 periods: the mean period was ∴ = 69·77 days. 1878, Aug. 19, to 1879, Dec. 24, six minima treated by the method of least squares gave the mean period = 70·00 days.

These results show that the period had a minimum value in the long interval from 1868, Sep. 12, to 1878, Aug. 19, and that it has since slightly increased; but as it might be doubted whether an error of one period might not be made in estimating the number of periods in so long an interval it may be stated that if it be assumed that only 51 periods elapsed during the interval the mean period would be 71·13 days, whilst 53 periods in the interval would give a mean of 68·45 days; but looking to the results for the nine years 1859-1868, and the two years 1878-9, it is evident that a mean period of 71·13 days is too long, and one of 68·45 days too short to be admissible.

The secondary minima I have observed from the discovery of the star's variability to the end of 1879 are as follows :—

No.	Date.	Mag.	Days in Interval.	Number of Periods.
1.	1859, Nov. 25......	8·6		
2.	1860, June 23......	8·7	211	3
3.	„ Sep. 10......	9·1	79	1
4.	1861, June 24......	8·8	287	4
5.	„ Aug. 30......	9·1	67	1
6.	„ Nov. 11......	9·2	73	1
7.	1862, Aug. 18......	9·1	280	4
8.	„ Oct. 28......	9·3	71	1

No.	Date.	Mag.	in Interval.	of Periods.
9.	1863, May 30......	9·2	214	3
10.	„ July 29......	9·1	60	1
11.	„ Oct. 15......	9·3	78	1
12.	„ Dec. 22......	9·2	67	1
13.	1864, May 25......	8·9	155	2
14.	„ Sep. 28......	8·8	126	2
15.	1865, July 9......	8·8	284	4
16.	„ Sep. 20......	8·9	73	1
17.	1866, July 5......	9·0	288	4
18.	„ Sep. 3......	8·9	60	1
19.	„ Nov. 15......	9·2	73	1
20.	1867, Aug. 28......	9·1	286	4
21.	„ Nov. 6......	8·9	70	1
22.	1868, Oct. 17......	9·4	345	5
23.	1878, July 16......	9·4	3559	51
24.	„ Dec. 5......	9·4	142	2
25.	1879, Sep. 3......	9·4	272	4
26.	„ Nov. 14......	9·3	72	1

Treating these data by the method adopted with the principal minima the resulting mean period = 70·173 days; and the epoch 1866, June 20·929.

Comparing these elements with those derived from the principal minima, it will be seen that the mean interval between a principal minimum and the following secondary minimum is 34·886 days, or almost exactly half the whole period.

My observations have enabled me to determine the times of 24 principal, and 19 secondary maxima, and comparing them with the times of the preceding principal minima I find that the mean intervals are respectively 14·8 and 45·6 days. Dr. Schönfeld in his *Zweiter Catalog von veränderlichen Sternen,* gives the mean intervals derived from his own observations in 1865-70 as follows :—

First maximum......14·4	14·8
Second minimum ...34·6	34·9
Second maximum ...45·3	45·6

Considering the nature of the observations, and the limited number of minima and maxima compared, the near agreement between the two sets of results may be regarded as satisfactory.

The mean magnitude of the variable in its

Principal minimum is 9·75
First maximum.........8·55
Second minimum ......9·08
Second maximum......8·68

An examination of the columns of magnitudes shows that considerable irregularities in the light of the star take place in both minima, the range of variation at the principal minimum being 1·2 mag., and at the secondary minimum 0·8 mag. The range of variation at both maxima is 0·7, the highest magnitude at the first maximum having been 8·3 and the lowest 9·0; while at the second maximum the respective magnitudes were 8·4 and 9·1.

Grouping the magnitudes at each minimum into three groups, and taking the means, we have the following results :—

Principal Minimum.			Secondary Minimum.			
Group.	Number of Mags.	Mean Mag.	Group.	Number of Mags.	Mean Mag.	Differences of Means.
1	12	9·81	1	9	9·01	0·80
2	12	9·75	2	9	8·99	0·76
3	11	9·68	3	8	9·26	0·42

The numbers in the last column seem to indicate that the slowly decreasing; and it will therefore be interesting to watch whether this decrease will go on until the difference entirely disappears and the star becomes a single period variable; or whether the difference is subject to periodical changes, alternately increasing and decreasing within certain limits.

Ordinary Meeting, March 9th, 1880.

J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"Notes on the Biquadratic equation

$$x^4 + nx^3 + ax^2 \pm bx + c = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots(1),"$$

by **Robert Rawson**, Hon. Member of the Manchester Literary and Phil. Society, Associ. of the Institution of Naval Architects, Member of the London Mathematical Society.

1. This equation has received the earnest attention of the greatest Mathematicians of Europe during the last three centuries. There is one principle, however, common to all their researches on this subject, viz., the formation of a biquadratic, with indefinite coefficients, the roots of which can be determined by means of cubic and quadratic equations. The form (1) can be readily reduced to the following form:

$$x^4 + ax^2 \pm bx + c = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

where a, b, c are real numbers. Under this form the biquadratic has been considered by nearly all writers on the subject.

The various forms which have been successfully proposed for the formation of the biquadratic, with indefinite coefficients, may be arranged in the following manner:

By Harriot.

$$(x-\alpha)(x-\beta)(x-\nu)(x-\delta) = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots(\text{A})$$

By Des Cartes, Bombelli, Oughtred, and Harriot.

$$(x^2 + \alpha x + \beta)(x^2 - \alpha x + \nu) = 0 \quad\ldots\ldots\ldots\ldots\ldots\ldots(\text{B})$$

By Ferrari, Newton, Waring, Maclaurin, and Simpson.

$$(x^2 + \delta x + \alpha)^2 = m(\sqrt{\beta}.x + \sqrt{\nu})^2 \quad\ldots\ldots\ldots\ldots\ldots\ldots(\text{C})$$

By Euler.

$$x^4 = (\alpha + \beta + \nu)^4 \quad\ldots\ldots\ldots\ldots\ldots\ldots(\text{D})$$

The quantities α, β, ν, δ are indefinite constant, which may be determined so as to satisfy four given conditions.

2. The form (A), according to Wallis, was first introduced by *Harriot*, a distinguished English mathematician at Oxford in the middle of the 16th century. He was the friend and tutor of Sir Walter Raleigh.

In consequence of the very general application of this method of forming algebraical equations of all dimensions, Wallis speaks of it in terms of high commendation. The following opinions are gathered from his Algebra, published in 1685:—

"Wherein lyes the main mystery of Algebra." (Wallis' Algebra, page 128.)

"In this method of producing higher equations Harriot is followed by Des Cartes." (Wallis' Algebra, page 140.)

"Which is the great key that opens the most abstruce mysteries in Algebra." (Wallis' Algebra, page 199.)

"I can hardly expect any new improvement of pure Algebra, other than what is built on the foundation laid by Harriot, and assumed by Des Cartes." (Wallis' Algebra, page 213.)

This is certainly high praise in favour of Harriot and his discoveries in Algebra by the foremost thinker in speculative mathematics before the Newtonian era.

It may be, however, fair to mention that both the French and Italian Algebraists disagree with the above view of the subject, and accuse Wallis of possessing an unfair partiality for the inventions of his countryman Harriot.

The truth seems to me to be this. The revival of learning in Europe in the 15th century no doubt had considerable power in stimulating independent thinkers in the fields of pure and mixed science. It is, therefore, only natural to suppose that publication should disclose some remarkable coincidences in the results of such independent investigations.

This conclusion is in strict accordance with the experience of our own times. Here, then, is the solution of the controversy respecting the first solution of biquadratic equations; and it is not unreasonable to infer that the first solution of a biquadratic known to English students, through the excellent treatise on the theory of equations by Todhunter, as Des Cartes, may justly be claimed by Bombelli, Oughtred, and Harriot.

In Wallis's Algebra, published in 1685, there is a copious account of the works of Vieta's specious arithmetic, Oughtred and his Clavis, Harriot's Algebra, published in 1631, Dr. Pell's Algebra, and several other works of European interest.

By multiplying out the form (A) there results

$$\begin{aligned} x^4 - (\alpha + \beta + \nu + \delta)x^3 + (\alpha\beta + \alpha\nu + \alpha\delta + \beta\nu + \beta\delta + \nu\delta)x^2 \\ - (\alpha\beta\nu + \alpha\beta\delta + \beta\nu\delta + \alpha\nu\delta)x \\ + \alpha\beta\nu\delta = 0 \ldots\ldots\ldots\ldots(3) \end{aligned}$$

If this biquadratic is to coincide with (2) it can do so only by determining the indefinite coefficients α, β, ν, δ so as to satisfy the following equations:

$$\alpha + \beta + \nu + \delta = 0 \ldots\ldots\ldots\ldots(4)$$

$$\alpha\beta + \alpha\nu + \alpha\delta + \beta\nu + \beta\delta + \nu\delta = a \ldots\ldots(5)$$

$$\alpha\beta\nu + \alpha\nu\delta + \beta\nu\delta + \alpha\beta\delta = \mp b \ldots\ldots\ldots\ldots(6)$$

$$\alpha\beta\nu\delta = c \ldots\ldots\ldots\ldots(7)$$

These equations, though admirable in discovering the various relations amongst the roots themselves, are but of little use in finding each root in functions of a, b, c. For instance square (4) and compare the result with (5), then—

$$\alpha^2 + \beta^2 + \nu^2 + \delta^2 = -2a \ldots\ldots\ldots\ldots(8)$$

$$\alpha^3 + \beta^3 + \nu^3 + \delta^3 = \mp 3b \ldots\ldots\ldots\ldots(9)$$

$$\alpha^4 + \beta^4 + \nu^4 + \delta^4 = 2a^2 - 4c \ldots\ldots\ldots\ldots(10)$$

$$\alpha^5 + \beta^5 + \nu^5 + \delta^5 = \pm 5ab \ldots\ldots\ldots\ldots(11)$$

The value of $\alpha^n + \beta^n + \nu^n + \delta^n$ can be obtained by Newton's process in terms of a, b, c.

3. The form (B) which is said to be due to Des Cartes, but according to Wallis is equally due to Bombelli, Oughtred, and Harriot:

Multiply out, then,

$$x^4 + (\beta + \nu - \alpha^2)x^2 + \alpha(\nu - \beta)x + \beta\nu = 0 \ldots\ldots\ldots(12)$$

This equation coincides with (2) when the following equations are satisfied:

$$\left.\begin{aligned} \beta+\nu-\alpha^2&=a \\ \alpha(\nu-\beta)&=\pm b \\ \beta\nu&=c \end{aligned}\right\}\ldots\ldots\ldots\ldots\ldots\ldots(13)$$

These conditions give

$$\beta=\frac{a}{2}+\frac{\alpha^2}{2}\mp\frac{b}{2\alpha}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(14)$$

$$\nu=\frac{a}{2}+\frac{\alpha^2}{2}\pm\frac{b}{2\alpha}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(15)$$

And an *auxiliary* cubic

$$(\alpha^2)^3+2a(\alpha^2)^2+(a^2-4c)(\alpha^2)-b^2=0\ldots\ldots\ldots\ldots(16)$$

Now, if the three roots of (16) be represented by r, s, t

$$\therefore\quad\left.\begin{aligned} \alpha&=\sqrt{r} \\ r+s+t&=-2a \\ rst&=b^2 \end{aligned}\right\}\ldots\ldots\ldots\ldots\ldots\ldots\ldots(17)$$

Substitute these values in (14), (15),

$$\therefore\qquad \beta=\frac{r}{4}-\frac{(\sqrt{s}\pm\sqrt{t})^2}{4}$$

and $$\nu=\frac{r}{4}-\frac{(\sqrt{s}\mp\sqrt{t})^2}{4}$$

Then the form (B) becomes

$$\left\{x^2+\sqrt{r}.x+\frac{r}{4}-\frac{(\sqrt{s}\pm\sqrt{t})^2}{4}\right\}\left\{x^2-\sqrt{r}.x+\frac{r}{4}-\frac{(\sqrt{s}\mp\sqrt{t})^2}{4}\right\}=0\ldots(18)$$

By solving these two quadratics.

$$\left.\begin{aligned} 2x&=\sqrt{r}+\sqrt{s}\pm\sqrt{t}\quad\text{or,}\ -\sqrt{r}-\sqrt{s}\mp\sqrt{t} \\ 2x&=\sqrt{r}+\sqrt{s}\mp\sqrt{t}\quad\text{or,}\ \sqrt{r}-\sqrt{s}\pm\sqrt{t} \end{aligned}\right\}\ldots\ldots(19)$$

The upper sign applies when (b) is positive, and the lower when (b) is negative. The auxiliary cubic is the same for $x^4+ax^2+bx+c=0$ as for $x^4+ax^2-bx+c=0$.

The omission of the double sign of (b) by Mr. Todhunter and other writers on this subject render their solutions unnecessarily restricted to the solution of $x^4+ax^2+bx+c=0$.

It readily follows from (19) that when the biquadratic equation $x^4+ax^2\pm bx+c=0$ has equal roots, the auxiliary cubic (16) has equal roots. (See Todhunter's Theory of Equations, page 115.) When the auxiliary cubic has equal roots it would not be safe to infer that the biquadratic has equal roots. In this case three roots only of the biquadratic are generally equal.

"If the cubic equation has one positive root and two negative roots, the biquadratic equation has two real roots and two imaginary roots, or else four imaginary roots." (Todhunter's Equations, page 115.)

It would seem from (19) that the four roots of the biquadratic are always imaginary in this case, except when the two negative roots of the cubic are equal; and then the biquadratic has two imaginary roots and two real and equal roots.

If the cubic has one positive root and two imaginary roots, the biquadratic has two real and two imaginary roots. (Todhunter's Equations, page 115.) This is readily proved by expanding $\sqrt{p+q\sqrt{-1}}, \sqrt{p-q\sqrt{-1}}$.

The sum of the squares of each root of (2) is equal to the sum of three roots of the auxiliary cubic.

4. The form (C) is said to be due to Simpson, Waring, and Ferrari, but it is certainly found in the *Universal* Arithmetic by Sir I. Newton, in Maclaurin's Algebra, as well as in Simpson's Tracts. It differs but little from the form (B), to which it is readily reduced as follows:—

$$x^2 + \delta x + a = \pm(\sqrt{m\beta}.x + \sqrt{m\nu})$$

$$\therefore \{x^2 + (\delta - \sqrt{m\beta})x + a - \sqrt{m\nu}\}\{x^2 + (\delta + \sqrt{m\beta})x + a + \sqrt{m\nu}\} = 0 \ldots (20)$$

This reduces by multiplication to

$$x^4 + 2\delta x^3 + (\delta^2 + 2a - m\beta)x^2 + 2(\delta a - m\sqrt{\beta\nu})x + a^2 - m\nu = 0 \ldots (21)$$

An equation which coincides with (1) when the following conditions are satisfied:

$$\left.\begin{aligned} 2\delta &= n \\ \delta^2 + 2a - m\beta &= a \\ a\delta - m\sqrt{\beta\nu} &= \pm\frac{b}{2} \\ a^2 - m\nu &= c \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots\ldots (22)$$

These equations give

$$m\beta = 2a + \delta^2 - a$$

$$m\nu = a^2 - c$$

With an auxiliary cubic

$$a^3 - \frac{a}{2}a^2 - \left(c \mp \frac{nb}{4}\right)a - \frac{cn^2}{8} + \frac{ac}{2} - \frac{b^2}{8} = 0 \ldots\ldots\ldots\ldots (23)$$

Newton and Maclaurin pass over this auxiliary cubic in silence, that is, their minds were evidently occupied with illustrating the method of surd divisors to such an extent that they did not obtain this cubic at all.

Simpson, following somewhat in the wake of Newton and Maclaurin, does indeed slightly refer to it as being an equation which "rises to the sixth dimension, and would perhaps require more trouble to reduce it than even the original propounded, little advantage would be reaped therefrom." (Simpson's Tracts, page 108.)

This view is surprising from Simpson, by far the greatest mathematician of his age. Had he used $\sqrt{\beta}$, $\sqrt{\nu}$ instead of β, ν, in all probability he would have found the auxiliary cubic referred to.

Emerson, writing twenty years after Simpson, in his solution of biquadratic equations, makes no use of Newton's method of surd divisors, a system which is never likely to be resuscitated in the presence of Horner's method of solving numerical equations of all dimensions.

5. The form (D) is entirely due to Euler alone, and, like many of the mathematical investigations of this wonderful man, it is characterised by great clearness, simplicity, and analytical beauty.

As there is no restriction on the constants α, β, ν, the values $\sqrt{\alpha}$, $\sqrt{\beta}$, $\sqrt{\nu}$ will be used instead.

The following operations are readily performed from (C):

$$x^2 = \alpha + \beta + \nu + 2(\sqrt{\alpha\beta} + \sqrt{\alpha\nu} + \sqrt{\beta\nu})$$

$$x^4 = (\alpha + \beta + \nu)^2 + 4(\alpha + \beta + \nu)(\sqrt{\alpha\beta} + \sqrt{\alpha\nu} + \sqrt{\beta\nu}) + 4(\sqrt{\alpha\beta} + \sqrt{\alpha\nu} + \sqrt{\beta\nu})^2$$

$$= (\alpha + \beta + \nu)^2 + 2(\alpha + \beta + \nu)\{x^2 - (\alpha + \beta + \nu)\} + 4(\alpha\beta + \alpha\nu + \beta\nu) + 8\sqrt{\alpha\beta\nu}.x$$

$$\therefore x^4 - 2(\alpha + \beta + \nu)x^2 - 8\sqrt{\alpha\beta\nu}.x + (\alpha + \beta + \nu)^2 - 4(\alpha\beta + \alpha\nu + \beta r) = 0 \ldots (24)$$

The coefficient of x in (24) is the only term affected by changing the signs of $\sqrt{\alpha}$, $\sqrt{\beta}$, $\sqrt{\nu}$. Therefore, when any two of these quantities are changed from positive to negative there will be no alteration in the biquadratic (24). Hence,

$(\sqrt{\alpha} - \sqrt{\beta} - \sqrt{\nu})$; $(-\sqrt{\alpha} - \sqrt{\beta} + \sqrt{\nu})$; $(-\sqrt{\alpha} + \sqrt{\beta} - \sqrt{\nu})$ are roots of (24) equally with $(\sqrt{\alpha}, + \sqrt{\beta}, + \sqrt{\nu})$.

It remains, then, to determine α, β, ν, so as to coincide with (2); for this purpose it is necessary that the following conditions should be satisfied:—

$$\alpha + \beta + \nu = -\frac{a}{2}$$

$$\alpha\beta\nu = \frac{b^2}{64}$$

$$\alpha\beta + v\nu + \beta\nu = \frac{a^2 - 4c}{16}$$

By the theory of equations α, β, ν, are the three roots of the auxiliary cubic—

$$z^3 + \frac{a}{2}z^2 + \frac{a^2 - 4c}{16}z - \frac{b^2}{64} = 0 \ldots\ldots\ldots\ldots\ldots\ldots(25)$$

The coefficient of (x) in (24) is positive when any one of the three terms $\sqrt{\alpha}$, $\sqrt{\beta}$, $\sqrt{\nu}$, or all of them negative.
Hence $(-\sqrt{\alpha} - \sqrt{\beta} - \sqrt{\nu})$;$-\sqrt{\alpha} + \sqrt{\beta} + \sqrt{\nu})$; $\sqrt{\alpha} - \sqrt{\beta} + \sqrt{\nu})$; $(\sqrt{\alpha} + \sqrt{\beta} - \sqrt{\nu})$ are the roots of $x^4 + ax^2 + bx + c = 0$.
It appears, therefore, that the introduction "of the ambiguities in sign" mentioned by Mr. Todhunter, page 120 of his Equations, is not necessary to establish Euler's results.

6. In Euler's process there appears considerable latitude for the exercise of the inventive faculty in the arrangement and magnitude of the roots.

The following occurred to me as being simple. Form the biquadratic as follows—

$$\left(x - \frac{\sqrt{\alpha}}{m} + \frac{\sqrt{\beta}}{m} - \frac{\sqrt{\nu}}{m}\right)\left(x - \frac{\sqrt{\alpha}}{m} - \frac{\sqrt{\beta}}{m} + \frac{\sqrt{\nu}}{m}\right)\left(x + \frac{\sqrt{\alpha}}{m} + \frac{\sqrt{\beta}}{m} + \frac{\sqrt{\nu}}{m}\right)$$

$$\left(x + \frac{\sqrt{\alpha}}{m} - \frac{\sqrt{\beta}}{m} - \frac{\sqrt{\nu}}{m}\right) = 0 \ \ldots\ldots\ldots\ldots\ldots\ldots\ldots(26)$$

$$\therefore \left\{\left(x - \frac{\sqrt{\alpha}}{m}\right)^2 - \frac{(\sqrt{\beta} - \sqrt{\nu})^2}{m^2}\right\}\left\{\left(x + \frac{\sqrt{\alpha}}{m}\right)^2 - \frac{(\sqrt{\beta} + \sqrt{\nu})^2}{m^2}\right\} = 0$$

Or,

$$x^4 - \frac{(\alpha+\beta+\nu)}{m^2}x^2 + \frac{8\sqrt{\alpha\beta\nu}}{m^3}.x + \frac{(\alpha+\beta+\nu)^2 - 4(\alpha\beta+\alpha\nu+\beta\nu)}{m^4} = 0. \quad (27)$$

Now this equation will coincide with $x^4 + ax^2 + bx + c = 0$ when

$$\alpha + \beta + c = -\frac{m^2a}{2}$$

$$\alpha\beta + \alpha\nu + \beta\nu = \frac{m^4(a^2-4c)}{16}$$

$$\alpha\beta\nu = \frac{m^6b^2}{64}$$

Hence, α, β, ν are the three roots of the auxiliary cubic

$$z^3 + \frac{m^2a}{2}z^2 + \frac{m^4(a^2-4c)}{16} - \frac{m^6b^2}{64} = 0 \ldots\ldots\ldots\ldots\ldots(28)$$

As this auxiliary is the same when (b) is negative, it will be necessary to change the signs of $\sqrt{\alpha}$, $\sqrt{\beta}$, $\sqrt{\nu}$, so as to make the coefficient of x in (27) negative—this is easily done when required. This mode of procedure renders unnecessary the solution of two quadratics.

7. The following, which has occurred to me, seems to combine all the advantages of the forms (B) and (C).

Assume—

$$\{x^2 + n\sqrt{\alpha}.x + \sqrt{\beta} + \sqrt{\nu}\}\{x^2 - n\sqrt{\alpha}.x + \sqrt{\beta} - \sqrt{\nu}\} = 0 \ldots(29)$$

The biquadratic formed by multiplying out.

$$x^4 + (2\sqrt{\beta} - n^2\alpha)x^2 - 2n\sqrt{\alpha\nu}.x + \beta - \nu = 0 \ldots\ldots\ldots\ldots(30)$$

The coincidence of (30) with (2) gives

$$\left.\begin{aligned} 2\sqrt{\beta} - n^2\alpha &= a \\ -2n\sqrt{\alpha\nu} &= \pm b \\ \beta - \nu &= c \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(31)$$

From these conditions it follows that

$$\sqrt{\nu} = \mp\frac{b}{2n\sqrt{\alpha}}$$

$$\sqrt{\beta} = \frac{a+n^2\alpha}{2}$$

With an auxiliary cubic

$$\alpha^3 + \frac{2a}{n}\alpha^2 + \frac{a^2-4c}{n^4} - \frac{b^2}{n^6} = 0 \ldots\ldots\ldots\ldots\ldots\ldots(32)$$

Let r, s, t, be the three roots of this auxiliary cubic.

Then, by the theory of equations

$$r+s+t=-\frac{2a}{n^2}$$

$$rst=\frac{b^2}{n^6}$$

$$\therefore x^2+n\sqrt{r}.x=-\sqrt{\beta}-\sqrt{\nu}=\frac{a}{2}-\frac{n^2r}{2}\pm\frac{b}{2n\sqrt{r}}$$

$$=-\frac{n^2r}{4}+\frac{n^2}{4}\left(\sqrt{s}\pm\sqrt{t}\right)^2$$

$$\therefore x=\frac{n}{2}\left\{-\sqrt{r}+\sqrt{s}\pm\sqrt{t}\right\} \text{ or, } \frac{n}{2}\left\{-\sqrt{r}-\sqrt{s}\mp\sqrt{t}\right\}$$

Similarly

$$x=\frac{n}{2}\left\{\sqrt{r}+\sqrt{s}\mp\sqrt{t}\right\} \text{ or, } \frac{n}{2}\left\{\sqrt{r}-\sqrt{s}\pm\sqrt{t}\right\}$$

These are, therefore, the four roots of

$$x^4+an^2\pm bx+c=0$$

8. There are other methods of solving biquadratics depending upon general views applicable to equations of all dimensions—

Tschirnhausen's method is to reduce

$$x^4+nx^3+ax^2+bx+c=0 \text{.....................}(33)$$

$$\text{to } y^4+Ay^2+B=0$$

by eleminating x between (33) and the following

$$x^2+\alpha x+\beta+y-0 \text{.........................}(34)$$

where α, β, are indeterminate constants.

Lagrange has applied the theory of symmetrical functions to the solution of equations of the third and fourth degrees.

The above methods effect the object in question, but, by processes far more complex than those adopted in this paper. Professor Young has given a very ingenious solution of a biquadralic. (Young's Equations, page 454.)

"The Castel Nuovo Lignite Deposit, near San Giovanni Tuscany," by WATSON SMITH, F.C.S., F.I.C.

During the Easter of 1877, I had been requested by Jas. Young, Esq., LL.D., F.R.S., to execute a commission in South Italy for him, and was on my return to him in Florence, when on passing a small station, "San Giovanni," still some

10 miles from Florence, I observed there at a siding a large quantity of apparently good *Lignite* piled up, and ready for delivery by rail. Further away I saw what appeared very like an ironworks, and this being at a little country place, with the beautiful hills of Vallombrosa in full view, led me to believe the *Lignite* deposit could not be far off. Reaching Florence I spoke of the circumstance to Dr. Young, and found he had already taken note of it, and he at once proposed I should go and investigate the matter, which I did. Proceeding to San Giovanni, and finding a guide, we drove out some four to six miles to a small village, "*Castel Nuovo*," from which the deposit, or rather workings, are about ten minutes walk distant. The country here is hilly, very verdant, and abounding in trees, but in the direction we came it is gently undulating, almost flat, and continues so to the Vallombrosa hills. The deposit appears to commence in the flat or gently undulating land, and stretch out amongst the low hilly district around *Castel Nuovo.*

The land surface covering the deposits, as far as yet investigated, extends to a superficies of about 4,000 by 7,000 square metres. This *is as far as yet discovered,* but there is more yet in all probability, the manager told me. The deposit, so far traced out, is calculated to last for 400 years yet. I was told the deposit was only discovered 11 years ago, and work was commenced (*i.e.* output) a few months after the discovery.

The entire deposit is owned by one company, and a tram is laid from the deposit to the railway station and the ironworks, owned by the same company, and close to the station mentioned.

The workings have at first sight the appearance of a great quarry, for the borings or tunnelling proceed longitudinally. Through the whole depth of the seam run five or six thin bands of greyish clay, 30 centimetres thick, they run quite horizontal and parallel to each other, with these also are to be seen one or two little bands of red ferruginous clay.

I should mention that the ironworks belonging to this company I was not able to visit, which I regret very much. However, as I saw no blast furnaces, and outside a good deal of "raw pig," I conclude that they busied themslves there merely with "*puddling.*" Most of the iron-ore, of best quality, used in this part of Italy comes from Elba.

The *Lignite* itself is not so far advanced in mineral character as the slaty variety known in Zürich, and occurring on the banks of the Lake by Meilen as "*Schieferkohl.*" It is not so hard and slaty. It is more fibrous or woody than this, and yet it is of a slaty texture. The bulk seems mostly a little wet, though a great deal appears very dry. It is all however exposed in the sun to dry. Some of the best is cut evenly in blocks and dried on drying flats of rough brick work, the fires underneath being maintained by some of the worst, with dust, smalls, and refuse. The colour of the Lignite is dark brown, much like the "Schieferkohl" of Meilen by Zürich.

In excavating the deposit, and detaching large blocks of Lignite, very frequently a large escape of gas occurs; there is a rush, and if torches are near, a flame, and then all is over again, the flame soon going out. From this it would appear that the fire-damp is pretty well diluted with Carbon dioxide, in all probability. However, no gas is ever met with, I was assured, which extinguishes candles. So it would appear that the "fire-damp" or "Marsh gas" is pretty uniformly in some excess, in the occluded gases. There is a good deal of sieving done, to separate dust from the coarser lumps. Now these sievings when thrown on one side in heaps, are said to "ferment" and take fire spontaneously. This is continually occuring and requires watchfulness. This fact points to the very probable occurrence of *iron pyrites* in a finely divided state, amongst the substance of the Lignite. I noticed in several places portions of blocks of woody matter quite charred by the combustion of gas, fired simply through intense heat, caused by oxidation of a freshly exposed surface of lignite.

With regard to the output in 1878, this was 700 tons per week, or 120 per day. 200 men were employed. Those working the deposit are paid at the rate of 1½ Liras *per diem*, the *lira* being about 9½d. of our money. So these are certainly not overpaid colliers!

The price of the dried Lignite is 12 Liras per 1000 kilos, which is not 10*s.* per ton, something near 9*s.* 6*d.* per ton.

On distillation, an aqueous acid liquid is obtained, smelling of wood-tar creosote. The tar smells like wood-tar. The pitch is bright, and very like wood-pitch. A kind of lubricant is made for the wheels of the tram wagons by mixing the crude tar with linseed oil. The gas obtained by distilling the lignite is to some extent illuminating, but not sufficiently so for house use. The manager said a process had been devised by a Florentine gentleman for conferring illuminating power on it, or of treating, or distilling it, so as to obtain illuminating gas from it sufficing for all purposes, and Florence was shortly to be lighted by this gas.

Possibly this method depends upon the fact mentioned by Percy in his work on "Fuel" (Metallurgy), p. 139, that "if the volatile products from the carbonization of wood be subjected to a considerably higher degree of heat than that usually adopted in the charring process, instead of CO_2, CO, and CH_4 being formed, a mixture of gases in which olefiant gas is now present, is obtained, and this possesses considerable illuminating power, and has been successfully applied for the purpose of illumination." I may just mention that in the ordinary distillation of wood in closed iron-retorts or ovens, just about the termination of the process, the gases evolved part of the works, in one case I knew of, but it only answered during the last few hours of the process.

"Analyses of the Ash of Wood of two varieties of the 'Eucalyptus' tree," by WATSON SMITH, F.C.S., F.I.C., Demonstrator and Assistant Lecturer in the Owens College.

During Easter, 1877, Jas. Young, Esq, LL.D., F.R.S., suggested to me that I should make analyses of the ash of the two varieties of eucalyptus, viz. E. rostrata and E. globulus, commonly known as red and blue gum trees. The pieces of wood I was supplied with by Dr. Young. They appeared to be stout branches, "crop-wood," and had the bark still on. The wood of E. rostrata ("red gum") was evidently closer and denser in texture than that of the "blue gum" or E. globulus. Its bark was also thinner and smoother. I took the samples of the wood to a joiner in Zürich, and requested him to plane them down to thin shavings, with the exception of a transverse section of each variety, to be reserved for specific gravity determinations. I requested him also to notice any peculiarities he might notice in the wood and report them, with his opinion of their quality. On calling subsequently, I noticed on entering the shop the odour of the gum or essential oil peculiar to the tree. The whole shop was fragrant with it. On planing the wood to shavings, this odour is quickly diffused around. The joiner stated it as his opinion that the wood of the "red gum" variety, especially, was extremely hard. It had quite turned the edge of his plane. The timber of such a tree, he said, would be of great value. The wood of the "blue gum" tree was a little behind in point of hardness and solidity. Determinations were now made of the *specific gravity* of these woods in what may be termed the *air-dried* condition.

The sections of both varieties had been placed in a dry atmosphere for one month. The results were:

Eucalyptus rostrata. "Red gum."		**Eucalyptus globulus.** "Blue gum."	
Spec. gravity	0·8112	Spec. gravity	0·772

These sections were now planed (bark included) to thin shavings, which were now weighed, burnt in a platinum dish to white ash, and this was weighed. The results were:

E. rostrata.		**E. globulus.**	
Ash	2·25%	Ash	2·01%

Analyses were now made of the ash obtained by burning the shavings of the larger samples. This ash was white, inclining to straw-colour.

Eucalyptus rostrata. "Red gum."		**Eucalyptus globulus.** "Blue gum."	
K_2O	9·50	K_2O	25·00
Na_2O	3·40	Na_2O	
MgO	6·30	MgO	6·47
CaO	43·80	CaO	35·08
Ferric and aluminic phosphates	0·78	Ferric and aluminic phosphates	1·07
MnO	trace	MnO	trace
SiO_2	0·29	Al_2O_3	trace
SO_3	1·57	SiO_2	0·34
Cl	0·60	SO_3	1·55
Sand and carbon	1·77	Cl	0·85
		Sand and carbon	1·04

The leaves of the eucalyptus contain very considerable quantities of tannin; in fact, I think it is certain at some time or other they may prove a valuable source of tannin if the tree be largely cultivated. I have been told that in some parts where the tree abounds, a kind of decoction is made of the leaves, and drunk as tea is.

General Meeting, March 23rd, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Dr. Lloyd Roberts, of Kersal Towers, Higher Broughton, was elected an Ordinary Member of the Society.

Ordinary Meeting, March 23rd, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

"On the History of the Word 'Chemistry' or 'Chemia,'" by Dr. R. ANGUS SMITH, F.R.S., &c.

The Author said that he had been led to a renewed consideration of the meaning of the word chemistry because of papers read lately on the subject by Professor Schorlemmer, F.R.S., in Manchester, and Mr. McTear in Glasgow, a very brief abstract of the latter being seen. He had always been inclined to agree with Olaus Borrichius in his reference to Egypt for the word chemia and the chemical arts as well as the early attempts at a science, and he was especially inclined to do so on reading of the few Greek manuscripts that have been published or described by sufficient extracts being given. The eyes of the writers were evidently much turned to the land of Chemi, whilst Isis and Osiris were favourites. For many years he had occasionally tried to interest young Greek scholars in the subject, and he had hoped that when Dr. Kopp began his description in his very learned work the "Beiträge zur Geschichte der Chemie" that the examinations would have been exhaustive, but that author was not inclined to go further at the time.

If Formicus used the word chemia in the second, third, or fourth century, and if chemistry were meant, we have a good starting point; but Formicus was an astrologer, and he gives no definition.

The tract by "Democritus" speaks of several chemical subjects, and he is said to have treated of metals and purple dyeing, having learnt Egyptian and other Eastern wisdom. Dr. Kopp considers that he may be referred to the fourth century. No criticism of any point of Kopp's enquiries was proposed; it was intended to begin where he had ended his enquiries, and to go into earlier times.

The old belief that chemistry has something to do with the soil of Egypt was not considered just. Brugsch says that Egypt is called black in contradistinction to the Desert, which is called red in hieroglyphic inscriptions. But the soil is not really black, and even if it were so, chemistry can have no direct relation to it. Still an art might receive the name of the country it came from, as Japan gives us japanning, &c. But so much came from Egypt to the West that we have little reason to suppose that such an obscure subject as chemistry would be the only one to take up the name of such an illustrious country and carry it to the West, whilst only those outside who foresaw the greatness of the Science would give it a great name. The Egyptians themselves would scarcely do so. Why should they choose this as peculiarly characteristic of their country?

Dr. Angus Smith came to the idea that the word חֵמָה Hema or Khema, meaning heat in Hebrew, was much more likely to be the origin of the name *chemia* than any hitherto fixed upon, and hoped by the later researches in Egypt and Assyria to have some light thrown upon it. Professor Theodores, of Owens College, being asked, gave information which confirmed him in the belief, and also showed that the word was not an Arabic one. "If *Kimia* be an Arabic word the attempt to graft it on hot or black is futile, and he quotes Bochart's proposal to derive it from *Kama*, to conceal, but wisely sets such aside. To quote one of his letters again—"חמם (cognate in character to יחם) means to be hot; from it are derived the forms Hammah, "heat," also sun, "the receptacle of heat." Another noun is Hema, "fiery anger." This latter form occurs in Deuteronomy 32, 24, in connection with the names of snakes or some such animals." This word is, according to Professor Theodores, incorrectly translated "poison." It would appear to be *heat*, as in *hot with rage.* The author did not know how far Semitic scholars would allow the next stage, which is to connect this with הָמָה, which certainly softens the first letter, giving us the meaning of *noise and excitement as by intoxicating drink.*

On enquiry of Dr. Birch how such a word could be connected with the Egyptian and Assyrian, he says, "The word for heat is *Hhamamu* or *Khamamu.*" Then he gives *qam,* Egypt, or the black land, *qam* meaning in Egyptian *black.*

Having obtained the first idea, *heat,* it was impossible to avoid going thus far, but there was a difficulty, namely, in the connecting links between Egyptian and Semitic. Still it is allowed that there is frequent connection, and this word links itself with the Hebrew both towards Egypt and Assyria. It was too early to stop here, because we have the province of Egypt, Chem, which would seem to have been the kernel of the Egyptian nationality, the nation keeping, as it expanded, the original name. The metropolitan name of Chem is Chemmis, although Thebes overshadowed it. In Brugsch's "Egypt from the Monuments,"* we learn that the district was especially under the god Chem, who was the divine representative of heat as emanating from the sun and producing life in all nature—not the sun as the Glorious Apollo, but the sun, we might say in modern phrase, as an actinic agent. Chem was called Pan by Greeks and Romans from some inferior characteristics. Pan, like Chem, had his high and his undignified position. Chem was "the Lord of Coptos," to which place there was a road from the Red Sea, through the very hot district of *Hammamat* (still from heat), where stones for the temples were obtained, and where attention was paid to the discovery of gold and silver, and where Chem was held as "the master of the tribes which inhabit the valley," and "the Lord Protector of the mountain."* Many complaints were made of the heat of this valley, where wells were dug in the time of Rameses the Second, because others much older had been closed up. It was a burnt land, and it still preserves its ancient character. It is the earliest place where we know metals to have been studied. On the west of the Nile and opposite was Tini, famous for purple dye, another branch of chemistry. Tini, or *Thinis*, or *This* was the place where the earliest king *Menes* sprang from. In *Chem* we are in the land of Heat, where the God of Heat was worshipped, where metals were worked, and where other chemical processes were employed, and from which the first chemist, whose writings are clearly on chemical subjects, and whose drawings are clearly of chemical apparatus, sprang, viz., Zosimus the Panopolite, or Zosimus of Chemmis, for this is the proper name of his city. The translation of Chem into Pan has been misleading, and still more of Chemmis into Panopolis.

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* A History of Egypt under the Pharaohs, derived entirely from the Monuments, by Brugsch-Bey. Murray, 1879.

We thus see that chemistry has received its name from no trifling accident, but from that great natural agent which has made the science, and the character of which we are continually learning.

Prof. Theodores had quoted for the author Bunsen's "Die Stellung Aegypten's in der Weltgeschichte," v. 5, s. 2, where he says, "'Hem' or 'hêm,' Sieden glühen (*i.e.* to boil, to glow with heat) = Hebrew, ham," and the next part of the quotation must be kept in view—"h is related to S. The Egyptian Sôm has the same value, and with this the Teutonic Sommer. Sommer has some connection through Sun, Sonne." This helps us to bridge over some of the ground to be seen.

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* From Brugsch-Bey.

Sayce's Assyrian Vocabulary was then consulted in order to see if an Assyrian or Accadian origin could be found for the word. No Accadian word of the kind was found. We are then guided to Semitic Assyrian, which has—

Samsu, the sun.
Samu, the heavens.
Samas, the sun-god.
Khamamu, and } heat.
Khammu, }
Khamsa, fire.
Khamdu, light.

The word then had full force in the East beyond Egypt.

The examination of metals, dyeing, colour making, and distilling seem decidedly to be referable to Egypt. Distillation leads to alcohol readily, when juices of plants containing sugar are used, and still more so when sugar is added.

In Egypt there were drinks of great potency, but the fullest idea seems to have grown in this direction in Asia, where the Kh or Ch was much softened into h, and converted into S. This is done by laws not to be examined here, but received from those who have studied the tongues of the regions under view (see the quotation from Bunsen).

The study of intoxicating liquors was carried out more fully in Asia, where mysticism was most prevalent. It is probable that the love of the spiritual, the unseen, and the immortal, was among some tribes at an early period connected with intoxicating substances. This itself is a great field of historic enquiry full of interest.

We have seen how *huma* is connected with Sôm, and if we go far East we find the great Asiatic drink Soma used by the inferior gods to strengthen them in their fight with demons. Soma is the Sanskrit word for this drink, but there are many Semitic words the same in Sanskrit, and this evidently has the same root, which is seen in Egypt moving towards metals and general chemistry: it had more force in the far East in connection with the mystic drink, although the idea may have arisen in Egypt. The vessel into which the drink flowed, the receiver, was called *kama* (Indo-germanische Chrestomathie, by Ebel, Leskien, Joh. Schmidt and Aug. Schleicher). In old Bactrian the word appears as *haoma*, whilst in Russian we find it returned, in all probability, as *koumiss*, to more western lands. We have yet to learn fully the medical effects of the *asclepias acida*, or moon plant, from which the Soma was made. Ordinary alcoholic liquids made by fermentation are notwithstanding much condemned in the Institutes of Menu.

The connection of the earliest thoughts of civilization and the later is remarkably seen in the history of this word. The hope of happiness from intoxicating liquids is evidently the direction which theory took in the minds of some searchers for truth, and constant deception seems not to have cured men entirely. We have removed the intense faith in the delusion and retained the name of the substance, *i.e.*, *aqua vitae*, *eau de vie*, *uisge beatha*, all meaning water of life. The Germans simplified the word by calling it *burnt wine* (Branntwein) or brandy, and we have cut down the latter and Celtic form into whisky. This history of the theory is believed to be correct, although quite apart from the derivation of the word chemistry. The connection of chemistry with *moist* or *dry* was considered as too slight to form a basis for an opinion, and the superstition concerning a plant that made gold was simply held to have no solid place in the history. The belief that the word *kem* in Arabic, alluded to the fifth element, or *ether*, as one of the five existences spoken of in an important part of Indian philosophy, was rejected—but this abstract is already longer than usually allowed.

"On the Electrical Resistance and its relation to the Tensile Strain and other mechanical properties of Iron and Steel Wire," by WILLIAM H. JOHNSON, B.Sc.

It has long been known that there is a great variation in the tensile strain of various qualities of iron and steel wire, depending on the mode of manufacture and purity of the material used, and it occurred to the writer that it would be interesting to examine if this difference in quality was accompanied by a corresponding alteration in the electrical resistance.

If electricity is a mode of motion of the particles of matter, then, it is only reasonable to suppose that anything which constrains this motion will increase the electric resistance and *vice versa.* Thus an annealed wire should conduct electricity better than an *un*annealed and a hardened and tempered steel wire, whose particles are in a state of tension, worse than a bright wire; also from the analogy of copper wire any increase of impurities in the iron would probably augment the electrical resistance. The following experiments will be found to confirm these views.

A series of preliminary experiments were made, beginning with a pure iron, smelted and worked throughout with charcoal and ending with a highly carbonized steel wire.

The apparatus used was a Wheatstone bridge, with divided meter scale and reflecting galvanometer of low resistance. All the wires except the piano steel were drawn in the same way to No. 18½ and then either hardened and tempered, or annealed and finished by drawing one hole to No. 20 = ·038 inch. The length of wire tested electrically, varied from 8 to 3 meters; care was taken to select such a length that the resistance was about 1·Ohm., as then all four arms of the balance were equal. A thermometer was placed close to the wood drum on which the wire was coiled and the temperature noted at the time of each experiment.

The circuit was first closed by pressing a button and then connection with the galvanometer was made. By a suitable arrangement of keys the current was allowed to circulate through the wire such a short time that it could not sensibly increase its temperature.

The wire was tested for breaking-strain in an apparatus similar in principle to a steel-yard weighing machine, the weights could be added without any jar and the elongation recorded at the moment of rupture. The length tested measured always 10 inches between the jaws of the machine.

The diameter was measured by a decimal guage, manufactured by Elliott Bros., reading to $\frac{1}{1000}$ inch.

The tortion tests were made by gripping a wire in two vices 8 inches apart and turning one of them on to a suitable apparatus till the wire broke.

The results are given in table A. Charcoal iron is here seen to have the least electrical resistance, or about one half that of piano steel. We also notice a regular increase of resistance as the impurities augment. Charcoal iron is almost chemically pure, while dephosphorized iron, the next on the list, has some impurities which increase in ordinary iron wire. Again in the samples of steel, piano steel wire, which has the highest electrical resistance, contains two or three times as much carbon as mild steel, while steel wire samples 3*a* and 2*a* are intermediate in carbon as they are intermediate in electrical resistance.

Table A shows that the breaking strain and electrical resistance increase together.

A series of eight sets of experiments were made on various specimens of charcoal iron wire to determine the effect of annealing on the electrical resistance, but without definite result, as the alteration caused by annealing appears to be so small that it is difficult to distinguish it from variations arising from change of temperature.

Annealing diminishes considerably the electrical resistance of puddled iron wire, and has a marked effect on steel, the exception in the case of sample 3*a* being probably due to temperature.

The annealed samples were cut originally from the same piece as the bright, and the same was the case with the hardened and tempered, so they are identical in chemical composition.

As the electrical resistance of steel wire seemed from these experiments to increase very rapidly, the writer determined to investigate it more closely. A set of seven samples of cast steel wire were prepared, all of them manufactured in the same way, differing only in the amount of carbon and other impurities. These samples were drawn to No. 18½ and each coil divided, one half being hardened and tempered, then drawn to No. 20 and tested, the other half of each coil was annealed and drawn one hole to No. 20, then tested in the bright state and afterwards part was annealed and tested. The eighth sample was Bessemer steel drawn and tempered in the same way as the others. The tempering was done in melted lead at a constant temperature.

The results are given in table B.

The electrical resistances in this table, as in A, are the mean of two or more experiments differing very little amongst themselves; the method of experiment and apparatus was the same as used before.

The figures denoting breaking strain, elongation and torsion, as in table A are the mean of two experiments in each case. These two experiments gave results sometimes the same, at others differing 4 to 6 per cent in the annealed samples, 3 per cent in the hardened and tempered and 4 per cent in the bright wires, except in sample No. 6, where a difference of 20 per cent was observed. This probably accounts for the wide divergence of this sample.

These variations may seem large, but it must be remembered that there is an essential difference between an observation on electrical resistance and one on tensile strain. If we suppose the length L of wire under experiment divided into a great number of extremely short pieces l_1, l_2, l_3, etc., each one would have a slightly different breaking strain, b_1, b_2, b_3, etc., and electrical resistance, r_1, r_2, r_3, etc., depending on the physical structure of each length l_1, l_2, l_3, etc. Now the observed electrical resistance R of the wire L is the sum of all resistances of the parts l_1, l_2, l_3, etc., or $R = \Sigma(r)$, while the observed breaking strain is not the *mean*, but the *minimum* breaking strain of all the pieces l_1, l_2, l_3, etc. It is an experimental fact, that the longer the piece of wire under experiment, the lower will be the breaking strain.

Some of the results of the experiments are given in table B. Here we see again that the electrical resistance and breaking strain increase together in a pretty regular way, the variations observed probably being due partly to differences in temperature, but chiefly to the fact just explained, that in the electrical experiments we register a *mean*, while in the experiments on tensile strain we register a *minimum* observation. This law must be applied with due care and only where the samples have been drawn in the same way.

The mode of manufacture of the 8 samples of steel leads one to believe that there is a regular increase of carbon and other impurities from No. 8 to 1. The writer hopes shortly to analyse these wires, as the resistance of some of the samples is most remarkable, being no less than 3 times that of pure iron.

No attempt has been made to reduce the electric observations by calculation to some fixed temperature, as it seemed improbable that samples with such varied resistances would increase in the same proportion as pure iron.

Table B shows us that annealing diminishes the electrical resistance of bright steel wire some 1 per cent, while hardening and tempering increases the resistance of annealed steel about 5 per cent. Now we may observe that the greatest increase of electrical resistance, viz., 8·1 per cent, and of breaking strain resulting from hardening and tempering, is found in piano steel, thus clearly showing that any process like wire drawing or tempering, which makes the particles of steel more rigid and difficult to separate, increases the electrical resistance.

The torsion tests vary a good deal among themselves, but it is interesting to observe, from table B, that the mean breaking strain of hardened and tempered steel is to that of the same steel annealed in the inverse ratio of the number of twists in 8 inches. The electrical resistance of the samples of steel is seen from table B to be usually inversely proportional to the number of twists in 8 inches.

It is generally allowed that the heat conductivity of metals is nearly proportionate to their electric conductivity. If this relation between the thermal and electric conductivity holds good for different qualities of iron and steel, then the electric resistance is an important factor of all iron and steel plates used in the construction of boilers, fire boxes, etc. It is important that the plates exposed to the flame should conduct heat well, and one of the advantages claimed for steel over iron, is that as a thinner plate can be used, it allows more heat to pass than the thicker iron one.

Thus a ½-inch steel plate can replace a ⅝-inch iron plate, but our experiments lead us to believe that as steel conducts electricity worse than iron the thin steel plate may, after all, let less heat through in a given time than a thick iron one.

Suppose the iron plate has a resistance of ·90 Ohms. per meter gramme, and the steel plate a resistance of 1·11 Ohms., then a ⅝-inch iron plate will allow as much heat to pass per unit of area as a half-inch steel plate. If the steel plate be more highly carbonized, say of Siemen's Martin steel, then the ½-inch steel plate will allow no more heat to pass per unit of area than an iron plate $\frac{3}{16}$-inch thicker. There is not space here to discuss this subject fully, but there is no doubt of its importance, and it is probable the time will soon come when our leading boiler makers will electrically test their steel plates, perhaps by the induction balance, just as they now test them for ductility and tensile strain.

TABLE A.

Electrical and Mechanical Tests of various qualities of Iron and Steel Wires.

Quality.	Sample No.	Length.	Weight.	Resistance 1 Meter Gramme.	Temperature of Experiment	Breaking Strain per Square Inch.	Twists in Eight Inches.	Elongation %	Diameter.
		M'ts.	Grms.	Ohms.		Lbs.			Inch
Annealed Charcoal Iron....................	6a	8	47·403	·7875	13°	50,280	138·5	27·75	·039
Bright ditto ...	7a	8	47·391	·7852	13°·5	105,840	57	2	·038
Dephosphorized Iron Annealed	—	5	28·000	·8908	10°	63,504	189	23	·038
Annealed Iron	9a	6	35·040	1·0123	13°	66,740	134·5	25·5	·038
Bright ditto	8a	6	33·231	1·0696	11°·5	112,896	49·5	0·75	·038
Mild Steel Annealed.	5a	6	32·890	1·1140	12°·5	70,268	207	19	·038
Ditto do. Bright.	4a	6	33·291	1·1270	15°·5	98,784	46	1·25	·038
Annealed Steel.........	3a	8	46·286	1·2520	15°	95,256	149	16·25	·038
Bright ditto	2a	3·50	20·081	1·2470	14°·5	132,600	69	1·5	·038
Hardened and Tempered ditto............	1a	5·20	29·500	1·2832	13°·5	134,244	96	1·5	·038
Annealed Piano Steel	—	4	28·930	1·3422	10°·25	127,840	152	9·5	·043
Hardened and Tempered Steel (Piano).	—	5	35·230	1·4497	11°·25	287,540	75	2·62	·043

TABLE B.

Quality.	Sample No.	Electrical Resistance per Meter gramme.						Breaking Strain in lbs. per square inch.			Twists in 8 inches.			Elongation at moment of fracture.		
		Annealed.		Bright.		Hardened and Tempered.		Annealed.	Bright.	Hardened and Tempered.	Annealed.	Bright.	Hardened and Tempered.	Annealed.	Bright.	Hardened and Tempered.
		Ohms.	T.	Ohms.	T.	Ohms.	T.									
Cast Steel...	1	2·140	13°	2·157	13½°	2·225	13½°	109,335	166,785	208,330	63	16	23	19·50	1·50	1·75
„ ...	2	1·903	11¼°	1·913	11½°	1·980	11¼°	105,810	155,140	204,780	67	19	19	17·87	2·00	1·87
„ ...	3	1·560	10¾°	1·575	11°	1·641	10°	96,075	137,695	169,270	64	44·5	63	19·25	1·50	1·75
„ ...	4	1·519	11¾°	1·526	12°	1·597	12°	91,701	182,305	170,160	89	59·5	47	17·25	1·62	1·75
„ ...	5	1·450	11⅓°	1·465	12°	1·526	12°	90,493	135,785	167,015	95	77	64·5	18·50	1·75	1·75
„ ...	6	1·430	8¾°	1·444	10¾°	1·515	10½°	89,937	123,590	166,515	81·5	105	63·5	19·50	1·75	1·87
„ ...	7	1·417	9⅓°	1·437	11°	1·502	11°	88,174	130,549	158,110	76·5	37	49·5	19·12	1·87	1·50
„ ...	8	1·070	9⅓°	1·098	11°	1·158	10¼°	79,718	135,790	158,110	140	43	32	19·50	1·00	2·00
Total.........		12·489		12·615		13·144		751,243	1,117,539	1,402,290	676	401	361·5	150·49	13·79	14·24
Average ...		1·561		1·577		1·643		93,905	139,692	175,286	84·5	50·1	45·2	18·81	1·72	1·78
Per Cent....		100		101·08		105·25		100	148·76	186·66	187	110·93	100			

MICROSCOPICAL AND NATURAL HISTORY SECTION.

March 15th, 1880.

Charles Bailey, F.L.S., President of the Section, in the Chair.

Mr. **Joseph Sidebotham**, F.L.S,, sent specimens of some Helices and Bulimi, collected by himself at Mentone, South France, for exhibition, and the Secretary (Mr. Melvill) also read a communication from him on the subject. The principal form of interest was a Helix, to which Mr. Sidebotham had appended the name of Mentonensis. In the opinion of the author, this snail, which was only found by him in an isolated spot at Cap. Martin, near Mentone, was originally a hybrid between H. pisana (Müll) and H. virgata (L.), snails abounding in the locality, but that by perpetual interbreeding it had attained a developed specific form. It is characterised by being somewhat smaller than typical H. pisana, yellowish, and banded as in H. virgata or ericetorum, without any of the other tessellated markings which so characterise H. pisana, but mouth formed as in that species.

Mr. E. P. **Quinn** called the attention of the geologists in the Section to the fact at the present moment an unusual opportunity was occurring at Oldham Edge, for those interested in Sigillaria to visit the place, some gigantic specimens having been recently disclosed intact.

Dr. **Tatham**, of Salford, described a new form of Stephenson's Binocular, which has been recently introduced by Messrs. Baker, of Holborn:

The microscope being mainly intended as a working instrument, is of firm, rather than of elegant construction;

the stage is remarkably large and substantial, and is so firmly mounted that it affords efficient support for the hands during the manipulation of an object under the Microscope. The stand does not possess the usual axial joint; but inasmuch as that portion of the tubing which carries the eye-pieces is bent at almost a right angle with the perpendicular portion of the body which carries the objective, the axial inclination of the stand is unnecessary. The binocular prism is remarkably well ground and set, and under it, objects of considerable thickness, such as injected specimens, stand out in relief, much in the same way as they appear under the Wenham's binocular. The instrument is capable of being used binocularly with high, equally well as with low powers; the prism acts as an erector, and when used in combination with a "combined two and four inch objective," recently invented by Zeiss, it forms a most useful dissecting miscroscope.

Dr. Tatham bore testimony to the great comfort he has experienced in the use of this form of binocular, and augured for it an extensive sale, more especially as the price of the binocular, stand complete, does not exceed seven or eight pounds.

Mr. MARK STIRRUP, F.G.S., read some notes on the Mollusca of Blackpool—the more noteworthy species being

Scalaria Turtonæ.
Scalaria communis.
Cyprina Islandica.
Pholas crispata.

Mr. J. BOYD read some notes on the Haustellum of the Haustellata, with diagrams, and also exhibited some microscopical slides illustrative of the subject.

Ordinary Meeting, April 6th, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

E. W. BINNEY, F.R.S., F.G.S., said that in vols. XVI. and XVIII. of the Proceedings of the Society he had given accounts of a *Eucalyptus globulus* which he had planted in his garden near the sea at Douglas, Isle of Man. During the winter of 1878 and 1879 it suffered in its foliage and young branches to a considerable extent; but during the past winter, although the temperature of the month of December, in the Isle of Man, was lower than that of the same month in 1878, the tree has almost escaped damage, and is at the present time growing vigorously and giving out a strong odour throughout the surrounding air. It has not grown much in height during the last two years, as it is now considerably higher than the sea wall near to which it grows, but it has much increased in the diameter of its stem. Up to this time it has shown no signs of flowering

"Note on modified Chlorophyll from the leaves of *Eucalyptus globulus*," by EDWARD SCHUNCK, Ph.D., F.R.S.

Whoever has seen the *Eucalyptus globulus* growing must have been struck with the peculiar glaucous appearance of the foliage, such as few European plants show. I thought it would be of interest to ascertain whether this peculiar appearance might be in any degree due to the state in which the chlorophyll exists in the leaves. A very simple experiment however sufficed to prove that the peculiar appearance referred to is owing to a covering of fatty matter, such as is seen on fresh plums and other fruit, which, though exceedingly thin, is sufficient to modify the green colour of the leaf. On washing the leaves with a little ether the film of fatty matter disappears instantly, the leaves then appearing green like ordinary leaves. The ether leaves on evaporation a white semi-crystalline fatty residue which melts at a temperature much lower than that of boiling water, and is partly soluble in dilute alkaline lye boiling, so that it probably consists in part of some fatty acid. The leaves after washing with ether do not differ in appearance from other leaves. The alcoholic and ethereal extracts of these leaves show however a peculiarity, as regards the chlorophyll contained in them, which I have not observed in any other green leaf extract, though the same thing may have been observed by others and recorded in one of the numerous memoirs on chlorophyll, the whole of which I do not profess to have read. If a few of the smallest and latest formed Eucalyptus leaves from the tips of the branches are extracted with ether a green solution is obtained which shows the usual absorption bands of ordinary unchanged chlorophyll. If however a little of the extract contained in a tightly corked test tube be kept in a dark cupboard for several days it gradually acquires a yellowish tint and now shows absorption bands coinciding with those of so-called 'acid chlorophyll,' that is, of the modification which is produced at once by adding a few drops of an acid, such as acetic acid, to a solution of ordinary chlorophyll. The change in the spectrum produced by the action of acids consists in the disappearance of the chlorophyll band III, and the intensification of bands II and IV, which now more nearly approach band I in strength. An alcoholic extract of more fully developed but still quite fresh and vigorous leaves (I took for the purpose the pair next in order of development to those at the summit of the branch) showed the ordinary chlorophyll bands, though on attentive examination band IV was found to be a little more defined than usual. After being kept in the dark however for 24 hours of 'acid chlorophyll' as distinctly as did the ethereal extract after several days. An alcoholic extract of fresh grass after being kept in the dark for several days had not changed in the least, and still showed the ordinary chlorophyll bands. On exposing the extract of grass for a few hours to sunlight it gradually lost its green colour, became of a pale yellow, and then showed hardly a trace of absorption bands, the band I being only just discernible. On exposing the alcoholic Eucalyptus extract, after being kept in the dark for 24 hours, to sunlight, it also became much paler in colour, but the bands I, II, and IV*a*, were quite as distinct as before insolation, and in addition to these the broad band IV*b* between the lines E and F of the spectrum, which also belongs to the so-called 'acid chlorophyll,' now came out very neatly, it having been previously invisible on account of the great obscurity in that part of the spectrum. It appears then that an alcoholic or ethereal extract of Eucalyptus leaves undergoes even in the absence of light a change whereby the normal chlorophyll contained in it is converted into a substance which shows the same absorption bands as, and is perhaps identical with, 'acid chlorophyll.' I am inclined to attribute the change which takes place to the large amount of essential oil contained in the leaves and consequently in the extracts. Essential oils, it is well known, convert inactive oxygen into ozone, and ozone, according to Gerland, produces a change in alcoholic solutions of chlorophyll similar to the one I have described, whilst under the same conditions ordinary oxygen is without effect, which Sachsse seeks to explain by supposing that the ozone in Gerland's experiments led to the formation of some organic acid which reacted on and modified the chlorophyll. Fresh Eucalyptus leaves contain no acid soluble in water, for a watery decoction of the leaves does not redden blue litmus paper and remains neutral even on exposure to the air for some days; but the alcoholic extract shows after insolation a marked acid reaction. It would be interesting to ascertain whether alcoholic extracts of other leaves containing much essential oil behave in the same manner as Eucalyptus extract. I made the experiment with leaves from the orange tree, in which, as in Eucalyptus leaves, numerous oil cells may be seen under a lens, but the alcoholic extract, on being kept for several days in the dark, remained unchanged. After insolation it differed slightly from an extract of grass made at the same time and exposed along with it to sunlight, bands I and IV*a* remaining visible, while the corresponding bands of the grass extract had disappeared.

"On the Chemical Composition of the Ink on Letters and Documents as Evidence in Legal Cases," by WILLIAM THOMSON, F.R.S.E.

The ideas which I propose to bring before you are not entirely new. They are based on the examination of the ink on letters and documents as a valuable mode of investigation in civil and criminal law cases.

It frequently happens that circumstantial evidence of a very simple character which is often overlooked, might occasionally have the effect of conclusively proving the innocence or guilt of an accused person, or of pointing in some definite direction towards tracing the culprit. In civil legal cases the same class of evidence may prove equally useful.

If, for instance, a person be murdered on the highway and any weapon or instrument found with which the deed had been committed, it is needless to say that such implement would be carefully examined for name, mark, or number, or in fact any peculiarity by which it may be traced to its former owner. Again, recent foot-prints in the snow or in soft clay often present sufficient individuality about them to make them useful in the detection of crime, and these posed that a common substance such as the ink used in writing a letter or document, has any special individuality about it. All ordinary inks are black or nearly so, yet it is conceivable that the name and address of the writer of an anonymous letter may, under a given combination of circumstances, be contained within the black fluid with which the letter was written. If then a murderer has left his trace behind him in the shape of paper and ink, it seems to me not improbable that by a judicious use of the latter, some important information may be obtained respecting him which may ultimately lead to his capture. Some years ago the information obtainable by the examination of the ink on papers or documents would probably have been comparatively insignificant, owing to the fact that the number of different kinds of ink in the market at that time would be small, and the modes of preparation of the ink simple, and the materials used not specially subject to variation. At the present day however a large number of different articles, many of which are subject to variation, are employed in the manufacture of ink, and a large number of different inks are in general use, and from these reasons the testimony capable of being obtained by the chemical examination of the ink on letters or documents at the present time may, in some cases, prove to be of the greatest importance.

The reagents which I have found to act best in the testing of the ink on papers are nine, viz.

1. Dilute sulphuric acid.
2. Strong hydrochloric acid.
3. Slightly diluted nitric acid.
4. Sulphurous acid solution.
5. Caustic soda solution.
6. Cold saturated solution of oxalic acid.
7. Solution of bleaching powder.
8. Solution of protochloride of tin.
9. Solution of perchloride of tin

The method which I have adopted in applying these reagents is to moisten one or more strokes or letters of the writing with each reagent, and then to absorb by blotting paper the excess of fluid, a few seconds afterwards.

By thus treating the ink on different envelopes lately sent to me I find they give very diverse results; with sulphuric acid, for instance, the black colours of the different inks are changed, in some to bright crimson, in others to deep red, whilst some become blue, green, violet, and grey of different shades, and some remain practically unaltered; and when, as sometimes happens, the same or nearly the same colours are produced in two different inks by one reagent, the colours produced by another are very different, thus showing clearly that the letters were not written by the same inks.

The same kind of ink, sold by the same maker, but made at different times, also varies more or less in its behaviour with the reagents, as shown by the comparisons of three samples of Lyon's inks, sold in small penny bottles, which were bought at different places.

Differences between inks which give nearly the same reactions may sometimes be observed by noticing the lengths of time which the reagents require to bring about the ultimate changes and by the shades of colour through which the ink passes after applying each reagent. Changes also continue to go on gradually for days and weeks after the reagents have been applied. The colours and shades of the same colour can be much more distinctly seen by the aid of a good pocket lens.

It is evident that if two persons use ink made by the same maker at the same time the reactions would be precisely similar, but it is easy to understand that after such ink has been in use for some time, owing to the different habits of the users, each may acquire a distinct individuality. One, for instance, may have been more exposed to the air or the direct sunlight than the other, and some of the colouring matters present may thus have been more or less altered or destroyed. One person may have a habit of leaving his steel pen in the fluid, so that some of the iron may be dissolved, thus altering the character of the ink, whilst the other may not do so, or may employ a quill in writing. Again, some persons may allow their inks to dry up to a certain extent and then add to them any fluid which may be at hand, such as tea, coffee, wine, beer, water, &c., each of which would alter the character of the writing fluid, whilst others may use mixtures of two or more different inks, in different and characteristic proportions. One can therefore understand that many persons may have in their ink-bottles fluids which are so peculiar in chemical composition that they may have as much individuality about them, when treated with reagents, as the faces of their owners. I have tested the same ink on different kinds of paper, and the resulting shades of colour produced were identical in each case.

To make use of this mode of investigation it would be necessary to get the ink or inks used by a suspected person, or preferably some writing made by him at or about the same time as any letter or document in question and test the two side by side with each other. The resulting shades of colours may agree precisely and may thus tell strongly against the suspected person, or they may differ very much and so point towards exonerating him.

A case lately occurred in which the expert M. Chabot was called and gave evidence to the effect that the handwriting in a certain libellous letter was that of the person who was indicted as the writer of it. As a witness for the defence another person came forward and swore that he was the writer of the letter in question, and on that evidence the case was dismissed. One can, however, under some circumstances, understand that a suspicion of such a witness having perjured himself may be justifiable, and such a suspicion may possibly be removed from an innocent person by his producing for chemical examination a paper written about the same time and with the same ink as that said to have been used in writing the letter in question.

I have arranged on two sheets of paper the writings on envelopes of fifty different persons, lately sent to me. One sheet contains 24, written in Manchester and the suburbs, and the other contains 26 from London and the provinces; and from a minute inspection of these it will be observed that most of them are very different from each other, whilst no two give exactly similar shades of colour with all the different reagents.

PHYSICAL AND MATHEMATICAL SECTION.

April 13th, 1880.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

"Colorimetry, Part IV.," by JAMES BOTTOMLEY, D.Sc.

On the Colour Relations of Nickel and Cobalt.

For some experiments which I was making in colorimetry, I wished to obtain a solution which would absorb all the kinds of light in the same ratio, so that whatever sort of light we started with, after penetration through such a solution, it would remain the same in character; the only variation being a change in intensity. Hence through such solutions white surfaces would appear grey of various shades, verging towards blackness as the length of the column increased. Such a fluid we might call a soluble black. I am not aware of any single fluid that fulfils the above conditions. It might be said, why not use ink? but such specimens of ink as I have examined are bluish or violet on copious dilution. Moreover the colour alters with the degree of oxidation; also it seems to be colouring matter in suspension rather than in solution. I had some hopes of succeeding by mixing solutions of nickel and cobalt salts. On reference to the Phil. Magazine, vol. VI., page 15, I find that the colour relations of nickel and cobalt had been studied by Mr. Thomas Bayley with a view to the quantitative determination of these metals founded upon the complementary character of their colours. He states, "The fact will have been observed by chemists that solutions of nickel and cobalt salts are so far complementary in colour that when they are mixed together the resulting liquid, if moderately dilute, is hardly to be distinguished from pure water." After considering the nature of the absorption spectra of nickel and cobalt salts, he states, "If the spectra were exactly complementary, on superimposing the nickel spectrum upon the cobalt spectrum the dark part on the one would cover exactly the light part on the other. This, however, though nearly the case, is not exactly so......this is why the solution obtained by mixing strong solutions of nickel and cobalt is not grey, but reddish brown in colour." Some experiments which I made seemed to confirm the opinion of Mr. Bayley, the nickel solutions contained 0·05 grms. of $NiSO_4$ per cub. c. and the cobalt solution contained 0·05 grms. $CoSO_4$ per cub. c. A mixture consisting of 50 cub. c. of cobalt solution with 100 cub. c. of nickel solution contained in a white porcelain basin, seemed to be a grey tinted with pink in the shallower parts, and having a tendency to pass into a yellowish tint as the depth increased. I now poured the fluid into a tall glass cylinder covered externally with black cloth except a circular aperture of 8 mm. diameter at the bottom. When I looked through the column of fluid at a white surface, the colour was decided, resembling somewhat the pigment known as yellow ochre. Also with a less proportion of cobalt to nickel, namely 20 cub. c. of cobalt solution to 50 cub. c. of nickel solution, I still obtained a tint in which yellow seemed to predominate. Had I employed solutions so dilute that no colour was perceptible in the mixture, this would not strictly imply that the colours were complementary, but that the resulting tint was too feeble to produce the impression of colour, and if we filled a long tube with such a dilute solution the colour might again become manifest. Moreover, my aim was not to mix two coloured solutions so as to obtain a fluid which exercised no perceptible absorption of light, but to obtain a fluid which would exercise a considerable absorption subject to a certain condition. The following consideration seemed to me to render it hopeless to obtain a soluble black by nickel and cobalt only. A solution of cobalt when dilute is pink, but if we look through a considerable thickness or through a concentrated solution, the pink shows a tendency to pass into a scarlet. This shows that as the quantity of the salt increases, the ratio of the yellow to the red increases. The colour of the undissolved salt is brownish red, and the colour of the solution seems to approximate towards this as the concentration increases. Hence the colour of a solution of cobalt alters not only in intensity but also in kind as the amount of the salt is increased. On the other hand, the green of a solution of nickel varies in intensity, but does not seem to vary in character, at least in any marked manner, as the quantity of the salt increases. In order that it should be generally complementary in character to cobalt, any inconstancy in the ratio of the red to the yellow of the latter would require a corresponding variation in the ratio of the yellow to the blue in the former, and the tint ought to pass from an emerald green to a bluish green. As this does not seem to be the case, it would follow that even if we mixed nickel and cobalt so as to obtain a perfect grey for a column of definite length, a column longer or shorter than this would still retain some colour. My experiments seemed to indicate a deficiency of blue in the mixture, and this I thought might be supplemented by another salt. So I tried the addition of sulphate of copper. After some trials I got a solution containing in 1,000 cub. c. 7·275 grms. N_1SO_4, 4,868 grms. $CoSO_4$, and 11,468 $CuSO_4$, the solutions also contained 30 cub. c. of strong sulphuric acid; this I added to guard against any possible formation of sub-salts on copious dilution. This solution seemed nearer to what I wanted than a solution of nickel and cobalt only; it did not however appear wholly free from colour, and possibly a variation of the quantities might have given a better result; also the tint seemed to vary somewhat with the nature and intensity of the incident light. When in the failing light of approaching evening I held the containing bottle against the grey sky I thought that there remained a somewhat pinkish tint, whilst in the colorimeter when looking at an external white surface through a column sufficiently long to produce a perceptible absorption, I thought the solution had a bluish tint. When viewed against gas light it gave a greenish tint. Within the range of coloured fluids in Chemistry there may be some which if combined would yield a mixture absorbing all colours in the same ratio, so as to be truly a soluble black. The preparation of such a fluid would be an interesting problem in physics. It seems to me that we might also have such fluids which on spectral analysis would show not an absorption of all colours in the same ratio, but would be resolved into a violet and yellow, or an orange and blue, or red and green, or some other combination of colours of a complementary character.

Remarks on the Formulæ for the Intensity of Light that has passed through Absorbing Media, and on a Method of Experimental Verification.

In my last paper on colorimetry I pointed out that the function which expresses the connection of the intensity of light with the quantity of colouring matter is of the same form as the function expressing the relationship of the intensity and the length of the absorbing column, and if we accept Herschel's formula Σak^t for the latter relationship, then an expression of the form $\Sigma a\kappa^Q$ must be taken to express the former relationship. The connection of these two may be shown more directly than I indicated in my last paper. If we grant one of the laws, the other may be deduced from it as a corollary. Take for instance the law as given by Herschel,

$$T = a_1 k_1^t + a_2 k_2^t + \&c.$$

Now it is manifest that if q be the quantity of colouring matter per unit of length, we may write the above formula

$$T = a_1 k_1^{q\frac{t}{q}} + a_2 k_2^{q\frac{t}{q}} + \&c.$$

For $k_1^{\frac{1}{q}}$, $k_2^{\frac{1}{q}}$, $k^{\frac{1}{q}}$, &c., substitute new constants κ_1, κ_2, κ_3, &c. Then we way write

$$T = \Sigma a\kappa^{qt}$$

Since q denotes the quantity of colouring matter per unit of length and t the total length, we shall have $Q = qt$, when Q denotes the whole quantity of colouring matter, so that we finally deduce

$$T = \Sigma a\kappa^Q$$

As the basis of a method of colorimetry, I took the relationship, that the length of the column was inversely as the quantity of colouring matter present when the colour was made constant. It may be readily shown to be a consequence of the laws stated above. Suppose C to be the constant colour, then

$$C = \Sigma ak^{qt}.$$

The form of the equation shows that C is the sum of a number of constants C_1, C_2, C_3, &c., such that

$$C_1 = a_1 k_1^{qt}$$
$$C_2 = a_2 k_2^{qt}$$
$$\ldots = \ldots$$
$$C_n = a_n k_n^{qt}$$

whence we obtain

$$\log\frac{C_1}{a_1}=qt\log k_1$$
$$\log\frac{C_2}{a_2}=qt\log k_2$$
$$\ldots\ldots=\ldots\ldots$$
$$\log\frac{C_n}{a_n}=qt\log k_n$$

and by addition we obtain

$$\log\frac{C_1}{a_1}+\log\frac{C_2}{a_2}+\&c.=qt(\log k_1+\log k_2+\&c.)$$
$$\text{or } qt=\frac{\log\left(\frac{C_1C_2}{a_1a_2}\ldots\ldots\frac{C_n}{a_n}\right)}{\log(k_1k_2\ldots\ldots k_n)}=\text{constant}.$$

In my last paper I stated that the law of absorption of light given by Herschel appears to have been obtained *a priori*; I have not found in his memoirs any experimental confirmation of it. The form of the expression has a somewhat formidable appearance, inasmuch as it involves the measurements of infinite varieties of light. But suppose that in the formula Σak^t, k is the same for every species of light; then we may write $T=k^t\Sigma a$ or $T=k^tI$, if I denote the incident light. In such a case the emergent light will be of the same nature as the incident light, and will differ only in intensity. Suppose the incident light to be white, the emergent light will be a white of less intensity,—that is, will be a grey, approaching to blackness, as the length of the column increases. A fluid medium, affecting white light in this way, we might call a soluble black, and my aim, in seeking to obtain such a fluid, was to apply it to the confirmation of the law. In a previous note I state that I had tried to obtain such a body; what I got was not wholly satisfactory, but I thought that with it I might obtain some approximate results. The solution I used consisted of 500 cub. c. of the previously mentioned fluid with 500 cub. c. of distilled water.

The mode in which I proposed to operate was as follows: take two white lights of different intensities, say W_1 and W_2, and look at them through the liquid. Suppose the lengths of the columns when equality of intensity is obtained to be t_1 and t_2, then $W_1k^{t_1}=W_2k^{t_2}$

Suppose we alter the lengths of the columns and in a second experiment, we find,

$$W_1 k^{t_1'} = W_2 k^{t_2'}$$

by cross multiplication and elimination of the common factor W_1W_2 we obtain

$$k^{t_1}k^{t_2'} = k^{t_2}k^{t_1'}$$

or as we may write it

$$k^{t_1+t_2'} = k^{t_2+t_1'}$$

Then taking the logarithms of both sides and dividing by log k we get finally

$$t_1 + t_2' = t_2 + t_1'$$

In this way I proposed to test the law.

I took, as standard tints, a smooth surface of $BaSO_4$ and another of $BaSO_4$ and carbon, in the proportion of 10 grms. of $BaSO_4$ to 0·006 grms. of carbon; these materials were intimately mixed and the powder reduced by pressure to a flat surface. The colorimeters used were glass cylinders covered externally with black cloth, the circular apertures at the bottom admitting light being 8 mm. in diameter.

One experiment gave the following results. Length of column 22·2 c., standard tint W_1 ($BaSO_4$). I now attempted to get the same intensity when looking through the second cylinder at tint W_2 ($BaSO_4$+carbon). The mean of two trials gave 14·8 c. as the equivalent column. Hence we have the following results:

$$W_1 k^{22.2} = W_2 k^{14.8}.$$

I now made the length of the column over W_1 13 c. The equivalent column over W_2 was as the mean of two trials 7·5. Hence

$$W_1 k^{13} = W_2 k^{7.5}.$$

From these experiments we get as the sum of t_1 and t_2' 29·7 and the sum of t_1' and t_2, 27·8.

A second experiment gave

$$W_1 k^{13} = W_2 k^{6.55}$$

$$W_1 k^{22.2} = W_2 k^{15.45}$$

Here $t_1 + t_2' = 28{\cdot}45$ and $t_2 + t_1' = 28{\cdot}76$.

A third experiment gave

$$W_1 k^{16.8} = W_2 k^{10.65}$$

$$W_1 k^{14.8} = W_2 k^{8.5}$$

Here $t_1 + t_2' = 25{\cdot}3$ and $t_1' + t_2 = 24{\cdot}93$.

$$W_1k^{22.2} = W_2k^{14.9}$$
$$W_1k^{12.5} = W_2k^{6.2}$$

Here $t_1 + t_2' = 28{\cdot}4$ and $t_1 + t_2' = 27{\cdot}4$.

A fifth experiment gave

$$W_1k^{25.1} = W_2k^{16.65}$$
$$W_1k^{13.7} = W_2k^{6.85}$$

Here we have $t_1 + t_2' = 31{\cdot}95$ and $t_1' + t_2 = 30{\cdot}35$.

It seems to me that the above results are as favourable as might be expected considering the difficulties of the enquiry; and even if all external circumstances necessary for the successful completion of such experiments had existed, there would yet remain the difficulty of deciding about the equality of two grey tints. In such matters it is difficult to say where judgment ends and fancy begins.

That any discrepancies might be due to such a cause was shown by the following experiments:—I took the two cylinders and poured from one into the other with the intention of obtaining the same tint in both. In one trial I could not very clearly distinguish a column 16·4c. long from one 14·6c. long, and in a second trial a column 16·2c. long seemed to give the same tint as a column 14·6c. long. In the above experiments I used one eye only, namely, the right one.

I also made the following experiments:—I took as the standard of intensity W seen through a column 12·5c. long. On a former occasion I had made 6·2 as the equivalent column to be used with W_2. On the present occasion I thought 6·5c. gave a nearer result, so I took the column at this length. Now if the law of absorption of light be true, if we increase both columns by the same quantity, the intensities should again correspond. So I added 4c. to each, making one column 16·5 and the other 10·2. I thought that the tints were the same. I now made one column 20·5 and the other 14·2. Again I thought the tints were the same. Finally I made one column 24·5 and the other 18·2. The tints seemed again to correspond.

"Note on Three New Stars," by JOSEPH BAXENDELL, F.R.A.S.

Canis minor; it had a decided orange colour, and I estimated its magnitude to be 8·8. In the beginning of December it began to diminish in brightness, and on the 18th, 19th, and 20th of January its magnitude was estimated to be 9·8. It afterwards increased, and on February 2nd was of the 9·2 magnitude, but it has since again diminished slightly, and on the night of the 11th instant was of the 9·6 magnitude.

Its place from observations made at Lord Lindsay's observatory, Dun Echt, is

$$1879{\cdot}0 \quad \alpha = 7\text{h. } 34\text{m. } 45{\cdot}67\text{s.} \quad \delta = +8^\circ 39' 39{\cdot}6''.$$

The second new star was discovered on the 28th of January, 1880, when it was of the 8·7 magnitude. It is in the constellation Gemini, and precedes No. 1655+13° of the *Bonner Durchmusterung* 37·5s., and is about 7·3′ north. It is in a cluster of seven or eight very small stars, and is in the same field with a small star which I had for some time suspected of variability. It has a decided reddish colour. In the middle of February it began to diminish in brightness, and on the 24th it was noticed that its colour had changed and become a deeper red. Its brightness has since continued to diminish, and on the 11th inst. its magnitude did not exceed 11·0. It will, in all probability, prove to be a periodical variable, and will, in conformity with Algelander's system of nomenclature, be designated *V Geminorum.*

The third star is in the constellation Bootes, and was discovered on the 12th of March, 1880, when its magnitude was 9·4. In the latter part of the month it increased to 9·2 magnitude, but on the 11th instant it had diminished to 9·6. There is a small star near it of about 10·5 magnitude, which I mapped down some years ago, and have often seen since without noticing anything on the place of the new star. It has a very slightly reddish colour, and its brightness suffers more under contracted apertures of the telescope than that of any of the neighbouring stars. It precedes No. 2955+18° of the B.D. about 47·2 s., and is about 0′·4 south.

Annual General Meeting, April 20th, 1880.

J. P. JOULE, D.C.L., LL.D., F.R.S., &c., President, in the Chair.

Report of the Council, April, 1880.

The Treasurer's annual Account appended to this report shows that the balance against the General Fund Account has been reduced from £113 0s. 11d. on the 1st of April, 1879, to £57 0s. 2d. on the 1st of April, 1880; that there is now a balance of £56 10s. 5d. in favour of the Natural History Fund Account; and deducting the difference between these two sums from the Compounders' Fund, £125, there is a balance of £124 10s. 3d. in favour of the Society on the 1st April, 1880, against a balance of only £8 10s. on the 1st of April, 1879.

The number of ordinary members on the roll of the Society on the 1st of April, 1879, was 160, and three new members have been elected; six members have resigned, and the number on the roll on the 1st instant was therefore 157.

At the last annual meeting of the Society a resolution was passed urging on the Governors of Owens College the great importance of completing the buildings for a Museum of Natural History with the least possible delay. In reply to this resolution the following Minute from the Owens College was forwarded to the President:

"At a meeting of the Council held at the College on Friday, the 21st November, 1879,

"It was resolved:

"That the Treasurer be requested to inform Dr. Joule that the Council is deeply impressed with the importance of proceeding with the erection of the buildings necessary for the due accommodation of the Natural History Collections, so soon as an appeal can he made to the public for the necessary funds with a reasonable prospect of success."

The subject of the celebration of the Centenary of the Society is at present engaging the attention of the Council: they recommend the appointment of a Committee for the purpose of drawing up a report on the work of the Society since its foundation. Further proposals of the Council will be submitted to the Society in due course.

At the meeting of the Council held on the 18th of November, 1879, a resolution was passed directing the House Committee "to reconsider and report upon the terms on which the Scientific Students' Association is allowed the use of the Society's Meeting Room;" and at the following meeting, on the 16th of December, the Committee reported "that as the number of members in the Scientific Students' Association has increased more than twofold since the terms of £1 per meeting were agreed upon, a fresh arrangement should be made, and they suggested that £2 10s. per meeting would be a proper charge, of which 10s. should go to Mr. Roscoe, whose remuneration at present is quite insufficient." The Association, however, declined to accept these terms, and ceased to hold their meetings in the Society's Meeting Room at the end of December.

The following papers and communications have been read at the Ordinary and Sectional Meetings of the Society during the Session:—

October 7th, 1879.—"On an Extension of the Ordinary Logic, connecting it with the Logic of Relatives," by Joseph John Murphy, F.G.S. Communicated by the Rev. Robert Harley, M.A., F.R.S.

October 13th, 1879.—"On the Means by which Hydra swallows its Prey," by M. M. Hartog, M.A., B.Sc., F.L.S.

October 14th, 1879.—"On Colorimetry, Part III.," by James Bottomley, D.Sc.

October 21st, 1879.—"Notes on some Fossils from the Iron Mines of Furness," by E. W. Binney, V.P., F.R.S., F.G.S.

"On the 'Differential Calculus' of Du Bourguet," by Sir James Cockle, F.R.S., Corresponding Member of the Society.

November 4th, 1879.—"Screw Propulsion, Part II.," by Robert Rawson, Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society, Member of the Mathematical Society.

November 10th, 1879.—"Additional Note on Hydra," by Marcus M. Hartog, M.A., B.Sc., F.L.S.

"On an Undescribed Acinetan," by Marcus M. Hartog, M.A., B.Sc., F.L.S.

November 18th, 1879.—"On the Meteor of November 11th, 1879," by E. W. Binney, V.P., F.R.S.

"Recording Sunshine," by David Winstanley, F.R.A.S.

"On Some Notices in Classical Authors of the Action of Sun light on Purple Dye," by James Bottomley, D.Sc., F.C.S.

"On Specimens of Ophioglussum Vulgatum var. Ambigium," by Charles Bailey, F.L.S.

"On the Origin of the word Chemistry," by Carl Schorlemmer, F.R.S.

December 2nd, 1879.—"On a Peculiar Feature in the Water of the Well in Carisbrooke Castle, Isle of Wight," by Harry Grimshaw, F.C.S.

"Note on the Identity of the Spectra obtained from the different Allotropic Forms of Carbon," by Arthur Schuster, Ph.D., F.R.S., and H. E. Roscoe, LL.D., F.R.S.

"On Photographs of the ultra red portions of the Solar Spectrum," by Captain Abney, R.E., F.R.S.

December 16th, 1879.—"On a New Form of Marine Rain Gauge," by W. J. Black, Esq. Communicated by J. B. Dancer, F.R.A.S.

"On Screw Propulsion, Part III.", by Robert Rawson, Assoc. I.N.A., Hon. Member of the Manchester Literary and Philosophical Society, Member of the Mathematical Society.

"On the Anal Respiration of the Copepoda," by Marcus M Hartog, M.A., B.Sc., F.L.S.

January 13th, 1880.—"The Radiograph," by David Winstanley, F.R.A.S.

January 19th, 1880.—"On the Occurrence of Silene Gallica (L.) and its sub-species in Jersey," by J. Cosmo Melville, M.A., F.L.S.

"Bog Butter (Butyrellite), from Co. Galway, Ireland," by John Plant, F.G.S.

January 20th, 1880—"On the Rainfall adjacent to the Sweetloves Reservoir, Sharples, for 1879," by the Rev. Thomas Mackereth, F.R.A.S., F.M.S.

"A Curious Rainbow," by E. W. Binney, F.R.S., F.G.S.

January 27th, 1880.—"On Epidemic Cycles," by Arthur Ransome, M.A., M.D.

February 10th, 1880.—"Notes on a bore through Triassic and Permian Strata lately made at Openshaw," by E. W. Binney, V.P., F.R.S., &c.

February 16th, 1880.—"On the Land and Fresh Water Shells of Tasmania," by Mr. Thomas Rogers.

"On Flexible Sandstone," by John Plant, F.G.S.

February 24th, 1880.—"On the Long Period Inequality in Rainfall," by Balfour Stewart, LL.D., F.R.S., Professor of Natural Philosophy at Owens College.

"On a Form of Representing the Velocity at any point of an Incompressible Fluid under Conservative Forces," by R. F. Gwyther, M.A.

"Notes on some Quaternion Transformations," by R. F. Gwyther, M.A.

March 2nd, 1880.—"Results of Observations of the double-period Variable Star R Sagittæ," by Joseph Baxendell, F.R.A.S.

March 9th, 1880.—"Notes on the Bi-quadratic Equation $x^4 + nx^3 + ax^2 \pm bx + c = 0$," by Robert Rawson, Hon. Member of the Manchester Literary and Philosophical Society, Assoc. I.N.A., Member of the London Mathematical Society.

"The 'Castel Nuovo' Lignite Deposit, near Giovanni, Tuscany," by Watson Smith, F.C.S., F.I.C.

"Analysis of the Ash of Wood of two varieties of the Eucalyptus Tree," by Watson Smith, F.C.S., F.I.C.

March 15th, 1880.—"On some Specimens of Helices from Mentone, South France," by Joseph Sidebotham, F.L.S.

"Description of a New Form of Stephenson's Binocular," by J. F. W. Tatham, M.D.

"Notes on the Mollusca of Blackpool," by Mark Stirrup, F.G.S.

March 23rd, 1880.—"On the Origin of the word Chemistry," Part II.," by Dr. R. Angus Smith, F.R.S.

"On the Electrical Resistance and its relation to the Tensile Strain and other mechanical properties of Iron and Steel Wire," by William H. Johnson, B.Sc.

April 6th, 1880.—"On a Eucalyptus globulus at Douglas, Isle of Man," by E. W. Binney, F.R.S., F.G.S.

"On the Chemical Composition of the Ink on letters or documents as evidence in legal cases," by William Thomson, F.R.S.E.

Several of the above papers have been printed, and others passed by the Council for printing in the new volume of Memoirs.

The Council consider it desirable to continue the system of electing Sectional Associates, and a resolution on the subject will be again submitted to the annual meeting for the approval of the members.

	£	s.	d.	1879-80. £	s.	d.	1878-9. £	s.	d.
1879.—April 1.									
To Cash in hand				8	10	0	1726	4	3
1880.—March 31.									
To Members' Contributions :—									
Arrears, 1876-7, 1 at £2 2s.	2	2	0						
„ 1877-8, 1 at £2 2s.	2	2	0						
„ 1878-9, 14 at £2 2s.	29	8	0						
New Members' Admission Fees, 1878-9, 2 at £2 2s.	4	4	0						
„ „ „ 1879-80, 3 at £2 2s	6	6	0						
„ Half Subscriptions, 1878-9, 1 at £1 1s.	1	1	0						
„ „ „ 1879-80, 2 at £1 1s.	2	2	0						
„ Subscriptions „ 1 at £2 2s.	2	2	0						
Old Members „ „ 140 at £2 2s.	294	0	0						
				343	7	0	322	7	0
To one Associates' Subscription for Library, 1879-80		10	0						
To Sectional Contributions, 1879-80 :—									
Physical and Mathematical Section	2	2	0						
Microscopical and Natural History Section	—	—	—						
				2	12	0	5	4	0
To use of Society's Rooms :—									
Manchester Geological Society, to 31st March, 1879	30	0	0						
Manchester Scientific Students' Association, to 25th December, 1879	36	5	0						
				66	5	0	61	0	0
To Sale of Society's Publications				1	18	0	2	1	10
To Natural History Fund :—									
Dividends on £1225 Gt. Western Stock				59	19	6	60	1	3
To Bank Interest, less Bank Postage, to 31st December, 1879					14	7	10	1	7
				£483	6	1	£2186	19	11

1880.—April 1.—To Cash in Manchester and Salford Bank £124 10 3

April, 1880, Audited and found correct,

(Signed) R. S. DALE.
C. S. TRAPP.

PHILOSOPHICAL SOCIETY.

FROM 1ST APRIL, 1879, TO THE 31ST MARCH, 1880, WITH A COMPARATIVE FOR THE SESSION 1878-1879.

Cr.

1880—March 31.	1879-80. £ s. d.	£ s. d.	1878-9. £ s. d.	£ s. d.
By Charges on Property :—				
Chief Rent	12 13 0		12 13 6	
Insurance against Fire	12 17 6		12 17 6	
Property Tax	3 10 10		3 10 10	
Repairs, Whitewashing, &c.	18 2		3 14 0	
		29 19 6		32 15 10
By House Expenditure :—				
Coals, Gas, Candles, and Water	12 16 1		17 0 11	
Tea and Coffee at Meetings	19 4 3		18 18 10	
House Duty	6 7 6		6 7 6	
Cleaning, Brushes, Sundries	5 5 8		4 17 9	
		43 13 6		47 5 0
By Administrative Charges :—				
Wages of Keeper of Rooms	57 4 0		57 4 0	
Postages and Carriage of Parcels	17 4 0		20 7 2	
Attendance on Sections and Societies	11 19 0		14 11 6	
Stationery and Printing Circulars	10 4 1		12 12 6	
		96 11 1		104 15 2
By Publishing :—				
Printing Memoirs	30 1 6		50 9 6	
Printing Proceedings	47 17 0		57 11 0	
Wood Engraving and Lithographing	4 13 0		14 11 0	
Editor of Memoirs and Proceedings	50 0 0		50 0 0	
		132 11 6		172 11 6
By Library :—				
Binding Books	2 18 3		78 10 6	
Books and Periodicals	38 19 6		50 4 6	
Assistant in Library	11 10 0		12 4 0	
Geological Record for 1877	10 6		10 6	
Palæontographical Society for 1880	1 1 0		1 1 0	
Ray Society for 1880	1 1 0		3 3 0	
		56 0 3		145 13 6
By Natural History Fund :—				
Investment of Capital Fund			1489 18 0	
Brokerage on ditto			7 9 0	
Legal Charges			41 12 0	
Works on Natural History			36 9 11	
Grant to Section for Books			100 0 0	
				1675 8 11
By Balance		124 10 3		8 10 0
		£483 6 1		£2186 19 11

	1879-80. £ s. d.	£ s. d.
Compounders' Fund :—		
Balance in favour of this Account, April 1st, 1880		125 0 0
Natural History Fund :—		
Dividends as above	59 19 6	
Less Balance, April 1st, 1879	3 9 1	
Balance in favour of this Account, April 1st, 1880		56 10 5
		£181 10 5
General Fund :—		
Balance against this Account, April 1st, 1879	113 0 11	
Expenditure, 1879-80, as above	358 15 10	
	471 16 9	
Receipts, 1879-80, as above	414 16 7	
Balance against this Account, April 1st, 1880		57 0 2
Balance in favour of the Society, April 1st, 1880		£124 10 3

On the motion of the Rev. JOSEPH FREESTON, seconded by Mr. JAMES SMITH, the Report was unanimously adopted and ordered to be printed in the Society's Proceedings.

On the motion of Mr. A. BROTHERS, seconded by Mr. R. S. DALE, it was resolved unanimously: That the system of electing Sectional Associates be continued during the ensuing session.

On the motion of Professor O. REYNOLDS, seconded by Mr. A. BROTHERS, it was resolved unanimously: That the following gentlemen be appointed a Committee to draw up a report on the work of the Society since its foundation:— Mr. Binney, Dr. Joule, Dr. Smith, Rev. W. Gaskell, Dr. Schunck, Dr. Bottomley, and Mr. Nicholson, with power to add to their number.

The following gentlemen were elected officers of the Society and members of the Council for the ensuing year:

President.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, D.C.L., LL.D., F.R.S., F.C.S.
EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
HENRY ENFIELD ROSCOE, B.A., PH.D., F.R.S., F.C.S.

Secretaries.

JOSEPH BAXENDELL, F.R.A.S.
OSBORNE REYNOLDS, M.A., F.R.S.

Treasurer.

CHARLES BAILEY, F.L.S.

Librarian.

FRANCIS NICHOLSON, F.Z.S.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
ROBERT DUKINFIELD DARBISHIRE, B.A., F.G.S.
WILLIAM BOYD DAWKINS, M.A., F.R.S., F.G.S.
BALFOUR STEWART, LL.D., F.R.S.
CARL SCHORLEMMER, F.R.S.
JAMES BOTTOMLEY, B.A., D.Sc., F.C.S.

Cockle, F.R.S., Corresponding Member of the Society.

The remarkable passage which (*Sup.*, pp. 9—10) I have translated from Du Bourguet conflicts with reiterated statements of Lagrange, who (*Leçons*, 1806, pp. 340—1), speaking of a certain equation, which is in substance the same as Du Bourguet's (330), and which expresses the criterion of integrability, says

"And this equation ought to hold in itself, that is to say, "to be identical, in order that the variable z may be capable "of being a function of x and y, and that, consequently, the "one proposed may have a primitive in x, y, and z."

Moreover, Lagrange says (*ib.* p. 341)

"But when the equation of condition does not hold in "itself, the proposed equation cannot subsist unless we "assume some relation between x, y, and z, in such manner "that two of these variables become functions of the third."

Again, speaking of a certain case in which the condition is not satisfied, Lagrange (*ib.* p. 342) says

"Thus it is impossible that z can be a function of x and y "considered as independent of each other."

Still Du Bourguet's conclusion from his example (a) may be easily verified. Under these circumstances, when we find Du Bourguet writing the following paragraph (vol. i., p. xxiij.),

"At the end of each volume I have placed a Table recapitulatory, article by article, of the subjects contained in the "volume; and in order to save accomplished mathematicians, who have scarcely the time to read the whole of an "elementary Treatise, the trouble of seeking for the articles "which may be of interest to them, I have marked in the "Tables recapitulatory of the two Parts, the articles the "most worthy of the attention of these mathematicians, by "one or two asterisks, according to the importance of the "subject."

it seems strange* to find no asterisk to Art. 361 at p. 590 (vol. ii.) of his copious Index, which might well be a model to authors of similar works.

2, Sandringham Gardens, Ealing, near London, W.,
April 9, 1880.

"On the History of the word Telegraph," by William E. A. Axon, M.R.S.L., &c.

There has been considerable interest manifested this year in the history of the word telegraph. Mr. Warren de la Rue states in *Nature* that the earliest instance he can find of its English use is in the edition of Johnson's Dictionary issued in 1818; and he quotes from Rees's Encyclopædia an account of the machines for signalling which were employed by order of the French Directory in 1793. There can be no doubt that we borrowed the word from the French, and the *Manchester Guardian* has recalled an instance of its early use in a literary form in this locality. "Tim Bobbin the Second" was the pseudonym of Robert Walker, of Little Moss, who wrote in the *Manchester Gazette* of about 1795 some dialogues in the Lancashire dialect with great natural humour and a very great power of faithful portraiture of the condition and speech of the poorer classes in the county palatine. These sketches were afterwards republished under the title of "Plebeian Politics," and one of the best known passages in it is that relating the story of the Saddleworth Shouting Telegraph. Walker had doubtless read in the newspapers of the time references to this wonderful invention of the French, but he was certainly one of the first to give the word a welcome to literature, assuming, of course, for the moment, that the debatable ground of dialectal writing is literature.

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* Nor does it seem less strange when, in his footnote at p. 596 of vol. ii., we find Du Bourguet saying "When, as in this chapter, nearly all the articles ought to be preceded by asterisks, we shall content ourselves with putting one or two asterisks, according to the importance of the subject, before the title of the chapter." He is here referring to chapter IV. of Section III, but it is in chapter IV. of Section II. that Art. 361 is contained. In a footnote at page 20 of vol. i. he refers to the *Théorie* of Lagrange, whom he alludes to (p. 212) or mentions (p. 483) subsequently in vol. i. and mentions in vol ii. (pp. 336, 471, 475, 481 and 502), but not in connection with the Proposition. The quotations in this communication are, of course, translations.—J. C.

The word "telegraph," unlike the great bulk of words, did not grow but was invented, and we have the testimony of its godfather as to the time, place, and circumstances of its baptism. Chappe was the inventor of an apparatus for the transmission of news by aerial signals, and had the usual difficulty of inventors. He haunted the office of the war minister in order to induce him to adopt the new system. One day whilst he was enlarging upon the advantages of his apparatus, which he then called *tachygraphe,* Miot de Melito observed that the word was badly chosen, and that he ought to change it to *télegraphe.* Chappe acknowledged the justness of this suggestion. Tachygraphy was a term that had long been used as a synonym for shorthand, whilst telegraph and telegraphy were quite new and fairly descriptive of the novelty. M. Maxime du Camp has given a very interesting account of the struggles of Chappe and of the genesis of his invention in the *Révue des deux Mondes,* 15 Mars, 1867, p. 464. Telegraphing by signals was known in an elementary fashion to the Romans. In England Robert Hooke invented a complicated arrangement for alphabetical signalling. Guillaume Amontons, who died in 1705, made experiments in the garden of the Luxembourg, which were witnessed by the Dauphin. Linguet, whilst a prisoner in the Bastille, invented a system of aerial signalling. Dupuis was the author of a method for the attainment of the same object, but he abandoned his plan in favour of that of Chappe. The inventor of the French telegraph, Claude Chappe, was born at Brulon (Sarthe), in 1763, and was destined for the priesthood. The seminary to which he was sent being about three quarters of a league distant from the school at which his brothers were receiving their education, he set about devising a method by which they might communicate with each other. Claude suggested that by means of the flat black rulers they might, upon the white exterior walls of the one school, describe figures that would be visible by the help of a *lorgnette* at the other. The figures, of course, had an arbitrary value previously agreed upon, and answering to certain phrases in their schoolboy vocabulary. This youthful freak had probably vanished from his mind when, as a man, he began to study the problem of distance-signalling, for his first thoughts were turned in the direction of electricity ; afterwards he tried combinations of colours, then experimented with sounds, and finally recurring to his early experience, invented the telegraph which became so efficient an aid to the armies of France. When electricity was employed nothing was more natural than to call the new apparatus the electric telegraph, and as it superseded the invention of Chappe the qualifying adjective became unnecessary, and the old name became applied solely to the new thing.

Mr. R. E. CUNLIFFE exhibited two slides of Fossil Diatoms from the London clay, discovered by Mr. Schrubsole, and mounted by Mr. Colo.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

Annual Meeting, April 12th, 1880.

Charles Bailey, F.L.S., President of the Section, in the Chair.

The Annual Report of the Council was read by the secretary (Mr. Melvill). It showed the Section to be in a very flourishing condition numerically, consisting at present of 38 Members, and 17 Associates, of which 1 Member and 6 Associates were elected during the past Session.

18 papers or other communications had been read at the meetings for the past Session, and the Council augured well from the increasing interest Members and Associates alike take in the proceedings, the average attendance being larger than in the 3 previous years.

They called the attention of all interested in the study of Natural History to the large additions that had been made during the past years to the Library—including as it does, the leading and standard works on most branches—and they would particularly call attention to such expensive works as Reichenbach's Flora Germaniæ et Helvetiæ, Reeve's Conchologia Iconica, Hewitson's Exotic Butterflies, and others.

The Council also earnestly invite all Members and Associates to favour them, at least once every Session, with either some paper, the exhibition of some object of Natural History, or microscopical slide. Whether new to science or not, it is highly important for the vitality of the Section that some subject should be found for discussion, and unfortunately the number of *contributing* Members and

Associates is in small proportion to the total number. It was therefore resolved, that in future the Secretary will, every month, call upon three gentlemen in alphabetical order in rotation to contribute something to the next meeting.

The Treasurer's Report was then read and passed.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

THE MICROSCOPICAL AND NATURAL HISTORY SECTION OF THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY IN ACCOUNT WITH THOS. H. BIRLEY, TREASURER.

DR.		£	s.	d.	CR.		£	s.	d.
1879.									
April 7,	To Balance due to Manchester and Salford Bank ...	12	0	11		By 40 Subscriptions at 10s.	20	0	0
10,	,, Wesley (Exotic Butterflies)	4	10	0		,, 9 Arrears at 10s.	4	10	0
Oct. 13,	,, Charles Simms (Printing)	2	14	0	1880.				
Dec. 16,	,, H. Stainton (Zoological Record)	1	0	0	April 8,	By Balance due to Bank	5	0	4
1880.									
Feb. 3,	,, Cornish (Microscopical Journal)	1	4	0					
	,, Van Voorst (Ibis)	1	1	0					
Mar. 31,	,, Subscriptions to Parent Society	2	2	0					
	,, Postage, Tea, Coffee, &c.	4	6	3					
1879.									
Dec. 31,	,, Interest charged by Bank	0	12	2					
		£29	10	4			£29	10	4

Examined and found correct,
JOSH. BAXENDELL.
JAMES COSMO MELVILL.

April 12, 1880.

The election of officers for the ensuing Session, 1880-81, then took place.

President:

ALFRED BROTHERS, F.R.A.S.

Vice-Presidents:

CHARLES BAILEY, F.L.S.
E. W. BINNEY, F.R.S.
R. D. DARBISHIRE, F.G.S.

Treasurer:

T. H. BIRLEY (Somerville).

Secretary:

J. COSMO MELVILL, M.A., F.L.S.

Council:

R. ELLIS CUNLIFFE.
J. BOYD.
JOSEPH BAXENDELL, F.R.A.S.
W. C. WILLIAMSON, F.R.S.
JOSEPH SIDEBOTHAM, F.L.S., F.R.A.S.
HASTINGS C. DENT.
Dr. ALCOCK.
W. BOYD DAWKINS, F.R.S., F.G.S.

List of Members and Associates:—

Members:

Alcock, Thomas, M.D.
Bailey, Charles, F.L.S.
Barratt, Walter Edward.
Barrow, John.
Baxendell, Joseph, F.R.A.S.
Becker, Wilfred, B.A.
Bickham, Spencer H., Jun.
Binney, E. W., F.R.S., F.G.S.
Birley Thomas Hornby.
Boyd, John.
Brockbank, W., F.G.S.
Brogden, Henry.
Brothers, Alfred, F.R.A.S.
Cottam, Samuel.
Coward, Edward.
Coward, Thomas.
Cunliffe, Robert Ellis.
Dale, John, F.C.S.
Dancer, Jno. Benjamin, F.R.A.S.
Dent, Hastings Charles.
Darbishire, R. D., B.A., F.G.S.
Dawkins, W. Boyd, F.R.S., F.G.S.
Deane, W. K.
Higgin, James, F.C.S.
Hurst, Henry Alexander.
Latham, Arthur George.
Marshall, A. Milnes, F.L.S., Prof. of Zoology, Owens College
Melvill, J. Cosmo, M.A., F.L.S.
Moore Samuel.
Morgan, J. E., M.D.
Nevill, Thomas Henry.
Nix, E. W., M.A.
Roberts, William, M.D.
Sidebotham, Joseph, F.R.A.S., F.L.S.
Smith, Robert Angus, Ph.D., F.R.S., F.C.S.
Williamson, Wm. Crawford, F.R.S., Prof. Nat. Hist., Owens College.
Wright, William Cort.

Associates:

Adams, Lionel.
Aitken, John.
Brockbank, Herbert W.
Cunliffe, Peter.
Hartog, Marcus M., B.Sc., F.L.S.
Labbey, B. B.
Linton, James.
Peace, Thos. S.
Percival, James.
Plant, John, F.G.S.
Quinn, Edward Paul.
Rogers, Thomas.
Stirrup, Mark, F.G.S.
Tatham, John F. W., M.D.
Ward, Edward.
Young, Sydney.

Mr. R. D. Darbishire gave some notes upon, and exhibited a case of eggs of the Black Back Gull (Larus ridibundus), found at Pilling Moss, near Fleetwood, Lancashire.

Mr. Darbishire also noted for record, that a pair of Rooks had built their nest in a poplar tree standing in Oxford Road, just beyond Ducie Street, about 1¾ miles from the centre of Manchester, and in the midst of a very dense district of small houses, Oxford Road itself being one of the noisiest and busiest streets of the whole city. The nest was built and a brood hatched last year. This year the birds were busy on 7th April.

Dr. Alcock read an introductory paper "On the Forms and so-called Species of Foraminifera."

Mr. R. D. Darbishire proposed the following resolution—"That this Meeting of the Microscopical Section of the Literary and Philosophical Society of Manchester respectfully invites the serious attention of the proper authorities of Owens College to the increasing disadvantage which students and naturalists of this district suffer, while the so-called Manchester Museum remains unexhibited and undeveloped in the private rooms of the College—while other towns, and notably Liverpool, enjoy very largely the benefits of free access to good and well arranged collections, Manchester alone offers no opportunity of the kind. The continued postponement of the promised development of the Museum constitutes, in the opinion of the Section, a grave departure from the spirit of the late Nat. Hist. Society's generous gift."

PHYSICAL AND MATHEMATICAL SECTION.

Annual Meeting, April 27th, 1880.

E. W. BINNEY, F.R.S., F.G.S., President of the Section, in the Chair.

The following gentlemen were elected Officers of the Section for the ensuing year:—

President
E. W. BINNEY, .R.S., F.G.S.
Vice-Presidents
JOSEPH BAXENDELL, F.R.A.S.
ALFRED BROTHERS, F.R.A.S.
Treasurer :
JAMES BOTTOMLEY, D.Sc., B.A.
Secretary :
REV. THOMAS MACKERETH, F.R.A.S., F.M.S.

"Colorimetry, Part V.," on the Absorption of Light by Turbid Solutions, by JAMES BOTTOMLEY, D.Sc.

Media containing colouring matter may be divided into two classes, transparent and turbid. It might be considered that in their behaviour with regard to light they were wholly dissimilar. But the question arises, is not the difference between transparency and turbidity one of degree.

Experience in the laboratory brings under our notice cases where matter is so finely divided as not to be separable from fluids by filtration, and showing but slight tendency to settle as a precipitate, and in some cases we have liquids which are apparently transparent, and yet are considered to hold solid matter in suspension. May not such examples be intermediate between solutions and cases where extremely fine particles are uniformly diffused through some transparent medium? If then we consider the passage from tranparency to turbidity as a continuous one, and if for a transparent fluid we have established some law of absorption of light, may not the same law be applicable to a turbid solution? The subject seemed to me interesting both as a scientific enquiry and on account of its application to quantitative analysis.

Suppose we have diffused through a liquid some finely divided solid matter; the action of such a turbid solution on light will be twofold, it disperses light and it absorbs light. By reason of the first action we are made aware of the colour of the turbidity; by reason of the second, any object seen through the liquid seems of diminished distinctness.

As a typical case, take carbon diffused through water. In a paper which I read at the last meeting of the Physical and Mathematical Section, I alluded to some attempts to obtain a soluble black, in order to make some experiments on the absorption of light. To assist my judgment as to the appearance which such a liquid should present, I had a cylinder containing a little carbon diffused through water. Weak diffusions of carbon in the colorimeter gave the same appearance when I looked at external white surfaces as I should have expected a liquid containing a soluble black in solution to give under the same conditions. A diffusion of carbon both disperses light and absorbs light. The dispersion gives rise to a greyish tint, due to the light coming from the innumerable particles of carbon. Owing to the absorption a white surface seen through the column of liquid seems of diminished whiteness. Hence, under ordinary circumstances, the light which comes to the eye has a twofold origin, part being transmitted light and part dispersed light. My first object was to dissociate these two phenomena. I therefore used cylinders which were covered with black cloth, admitting light by circular apertures at the bottom, 8 mm. in diameter. In this way the dispersed light was almost wholly cut off. In some cases a feebly nebulous light could be seen around the apertures, but it was very slight and did not interfere with experiments to determine the absorptive properties. With any attempt to explain by physical optics the analogy of the absorption of light by solutions and diffusions I have nothing to do, but on the supposition that there was continuity. I was led to expect that a function of the same form would express the intensity of the transmitted light in both cases. Also independently of such considerations it seemed probable from a reflection upon the mode of distribution of the particles throughout the mass of fluid, that the function would be the same as for transparent solutions. In the latter case, both experiment and reasoning from first principles concur in giving a formula. $\Sigma\alpha\kappa$ for the intensity of light passing through a column t units long. As I have pointed out in another paper, this implies an expression of the form $\Sigma\alpha\kappa,^{Q}$ denoting the connection between the transmitted light and the quantity of colouring matter. Therefore we might expect a similar expression to hold in the case of diffusions (the term seems to me more convenient than to speak of turbid solutions). If such be the case, one consequence will be that, if we have two cylinders containing in equal bulks of water Q and Q' of solid matter in suspension, if we adjust the columns so as to obtain the same intensity of light when we regard external white surface, then the lengths of the columns will fulfil the condition $Q_t = Q_t'$.

The carbon that I used was lamp-black recalcined in a covered platinum crucible. After being so treated it seemed blacker than it did before. Of this 0·4170 grams were ground up in a mortar with 10 drops of a solution of gum. The contents of the mortar were then rinsed out with water, and diffused by shaking through 500 cub. c. of water. This was poured into a cylinder; the length of the column was 22·5 c.m. After the lapse of 24 hours I drew off by a pipette the upper portion; the column so removed was 15·8 c.m. long. This carbon diffusion contained so much solid matter that I found it inconveniently strong for experiments, so as occasion required I made weaker diffusions, containing in 250 cub. c. 5, 10, 20, or 40 of the stronger diffusion. Of these weaker liquids portions were taken and mixed with water so as to yield a bulk of 500 cub. c. I stated above that the carbon was ground up with 10 drops of gum, so as to yield a smooth, thick consistence. The amount of dry gum would be very small, nevertheless it had a remarkable effect in increasing the adhesion of the carbon to the water. When I had shaken up carbon alone with water it had sensibly subsided after the lapse of a few hours. After the addition of so small a quantity of gum the tendency to deposit is much diminished. The following are the details of some experiments. The strength of the carbon diffusion is given in terms of the number of cub. c. of the strong carbon diffusions in 500 cub. c. of water. The external white surface was a sheet of white paper. In all cases I have used the right eye only. The number under A denotes the mean of two trials got by pouring into the cylinder, and therefore likely to yield too low results. B denotes the mean of two trials got by pouring out of the cylinder, and therefore likely to give too high results. C denotes the mean of A and B. D denotes the length required by theory.

Standard diffusion, 1·2 cub. c. in 500 cub. c. of water, length of column, 21·2.

Expt. I.—Comparison diffusion contains 1·8 cub. c. in 500 cub. c.

A	B	C	D
14·35	16·35	15·25	14·13

A second trial gave

A	B	C	D
12·65	14·65	13·65	14·13

Expt. II.—Comparison diffusion contains 2·4 cub. c. in 500 cub. c.

A	B	C	D
10·6	11·25	10·93	10·6

Expt. III.—Comparison diffusion contains 3·0 cub. c. in 500 cub. c.

A	B	C	D
8·4	9·25	8·83	8·48

Expt. IV.—Comparison diffusion contains 3·6 cub. c. in 500 cub. c.

A	B	C	D
6·85	7·6	7·22	7·07

Expt. V.—Comparison diffusion contains 4·2 cub. c. in 500 cub. c.

A	B	C	D
5·6	6·1	5·85	6·06

Expt. VI.—Comparison diffusion contains 4·8 cub. c. in 500 cub. c.

A	B	C	D
5·1	5·55	5·32	5·3

Expt. VII.—Comparison diffusion contains 5·4 cub. c. in 500 cub. c.

A	B	C	D
4·1	4·5	4·3	4·71

On another occasion I got a nearer result, as follows:

A	B	C	D
4·7	4·95	4·82	4·71

Expt. VIII.—Comparison diffusion contains 6 cub. c. in 500 cub. c.

A	B	C	D
4·13	4·58	4·36	4·24

Expt. IX.—Comparison diffusion contains 6·6 cub. c. in 500 cub. c.

A	B	C	D
3·98	4·25	4·11	3·85

Expt. X.—Comparison diffusion contains 7·2 cub. c. in 500 cub. c.

A	B	C	D
3·48	3·75	3·61	3·53

Expt. XI.—Comparison diffusion contains 7·8 cub. c. in 500 cub. c.

A	B	C	D
3·23	3·45	3·34	3·26

Expt. XII.—Comparison diffusion contains 8·4 cub. c. in 500 cub. c.

A	B	C	D
3·03	3·45	3·24	3·03

Expt. XIII.—Comparison diffusion contains 9 cub. c. in 500 cub. c.

A	B	C	D
2·9	3	2·95	2·82

Expt. XIV.—Comparison diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·83	3·05	2·94	2·65

Expt. XV.—Comparison diffusion contains 14·4 cub. c. in 500 cub. c.

A	B	C	D
1·98	2·08	2·03	1·77

Expt. XVI.—Comparison diffusion contains 19·2 cub. c. in 500 cub. c.

A	B	C	D
1·6	1·58	1·59	1·32

Many of the above numbers are close approximations. I also tried columns of the lengths given by theory in all experiments, except by the ninth, where by an oversight I neglected to do so. The tints so obtained were in every case satisfactory, until I reached Experiment XIV. In this and in the succeeding experiments I thought that columns of the theoretical lengths gave tints slightly lighter than the standard diffusion. On some occasions I have wavered in my opinion that the theoretical column gave too light a tint in the fourteenth experiment. At another time I got the following result:—

Expt. XVII.—Comparison diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·8	2·95	2·87	2·65

Here again the number under C is a little greater than that under D. I afterwards repeated the experiment with this variation—that the standard diffusion was contained in the comparison cylinder, and the comparison diffusion in the standard cylinder.

Expt. XVIII.—Standard diffusion contains 1·2 cub. c. in 500 cub. c.; length of column, 22·6.

Comparison diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·85	3·07	2·96	2·82

On another occasion I got—

A	B	C	D
2·85	3·15	3·0	2·82

Here again there still remains a tendency to make the column a little longer than the theoretical.

I repeated the experiments in which the strength of the comparison diffusion is several times a multiple of the strength of the standard diffusion. The strength of the standard diffusion was the same as in the previous experiments, and the length of the column was 21·2.

Expt. XIX.—Comparison diffusion contains 9·6 cub. c. in 500 cub. c.

A	B	C	D
2·63	2·98	2·8	2·65

When I actually tried the theoretical column I thought that the tint was perhaps slightly lighter.

Expt. XX.—Comparison diffusion contains 14·4 cub. c. in 500 cub. c.

A	B	C	D
1·9	2·15	2·02	1·77

The tint given by the theoretical column I thought slightly lighter.

Expt. XXI.—Comparison diffusion contains 19·2 cub. c. in 500 cub. c.

A	B	C	D
1·55	1·63	1·59	1·32

The tint given by the theoretical column I thought slightly lighter.

Expt. XXII.—Comparison diffusion contains 24·0 cub. c. in 500 cub. c.

A	B	C	D
1·15	1·3	1·22	1·06

The tint given by the theoretical column I thought slightly lighter, but hardly distinguishable.

Expt. XXIII.—Comparison diffusions contains 28·8 cub. c. in 500 cub. c.

A	B	C	D
1·0	1·1	1·05	0·88

The tint given by the theoretical column I thought slightly lighter.

Expt. XXIV.—Comparison liquid contains 33·6 cub. c. in 500 cub. c.

A	B	C	D
0·88	0·9	0·89	0·76

The tint given by the theoretical column I thought slightly lighter.

Expt. XXV.—Comparison liquid contains 38·4 cub. c. in 500 cub. c.

A	B	C	D
0·73	0·83	0·78	0·66

The tint given by the theoretical column I thought slightly lighter; they were, however, very nearly the same.

This second series of experiments confirms the result of the first series that there is a slight departure from the rule, when the strength of one diffusion is several times a multiple of the other. Both series show a tendency to make the column in the comparison cylinder slightly too long. We must not hastily conclude that in these cases the theory is not applicable. It seems to me that we should expect such a result, for the medium is not perfectly transparent, and in one cylinder we have a column of this medium (water) several times a multiple of the length of the column in the other. This would require some slight compensation. In these experiments I have taken the lower level of the meniscus as the proper reading. In some of the stronger diffusions it was a little difficult to determine this exactly. On the whole, I think that the above experiments are favourable to the assumption that for a column of fluid containing finely divided carbon in suspension the relationship $Qt =$ constant holds, if the intensity of the transmitted light remain constant. If we represent the above results graphically, and take as the theoretical curve the rectangular hyperbola $xy = 25{\cdot}44$, it will be seen that the results of the experiments do not depart far from the curve.

In a paper read at the last meeting of the section, I suggested a method for testing the assumed laws of the absorption of light. I also have applied the same reasoning and method of experiment to carbon diffusions. In one series of experiments I took a diffusion containing 1·934 cub. c. in 500 cub. c. The standard shades of grey used were one consisting of 10 grms. $BaSO_4$ and 0·012 of lamp-black—this I denote by Wa—the other consisted of 10 grms. of $BaSO_4$ and 0·048 of lamp-black—this I denote by Wb. The materials were well incorporated by shaking and grinding. To the powder I then added a little water so as to obtain a mixture of suitable consistence to be used as a paint. This was applied by a brush to pieces of cardboard so as to obtain uniform surfaces. These surfaces were then dried, the colorimeters employed were the same as in the last experiments. I looked at Wb through a column 3 c.m. long. I held the other cylinder over Wa and endeavoured to get the same tint. The mean of two columns, one probably a little too long and the other probably a little too short, gave 8·2 as the proper length. So I made the column of this length, I thought that the tints were the same. Now if the law hold with regard to turbid solutions, if we increase both columns by the same length, the tints will again correspond. I took 4 c.m. as the common increment. The lengths of the columns were made 7 and 12·2. I thought the resulting tints equal. The lengths of the columns were made 11 and 16·2. I thought they were about equal, possibly Wa slightly lighter. My impression varied a little with the illumination of the surfaces. The lengths of the columns were now made 15 and 20·2, the tints seemed about equal, possibly Wa slightly lighter. The difference if any must be small, as is shown by the following experiment. Taking Wb seen through a column 15 c.m. long, as the standard tint, I endeavoured to get the same tint with Wa seen through the other cylinder. For the lower limit I got 19·9 c.m., and for the upper limit 21·0 c.m., the mean of these is 20·45, not far removed from 20·2. Afterwards I thought a column of this length satisfied. I now made the lengths of the columns 19 and 24·2. I thought that the tints again corresponded.

Some experiments of a similar nature were made with a stronger diffusion, it contained 3·747 cub. c. in 500 cub. c. The tints of grey used were the same as in the last experiments. The standard tint at the commencement was Wb seen through a column 4 c.m. long. To get a similar tint with Wa one determination for the upper limit gave 6·5, and one determination for the lower limit gave 6·1. The mean of these is 6·3. A column of this length seemed to satisfy. The common increment is 3 c.m. The columns were made 7 and 9·3 c.m. long. The tints seemed to correspond. The columns were made 10 and 12·3 c.m. long. The tints again seemed to correspond. The columns were made 13 and 15·3 c.m. long. The tints seemed again to correspond. Finally, the columns were made 16 and 18·3 c.m. long. The tints again corresponded. In the two last experiments the greys obtained were very deep. I also made the following experiment. I took two grey tints: one consisted of 10 grms. $BaSO_4$ and 0·042 grms. of lamp-black. The other consisted of 10 grms. $BaSO_4$ and 0·4003 grms. of lamp-black. These greys were made into a paint by the addition of a little water, and pieces of cardboard covered with them. They were then dried. These we may denote by W_α and W_β. I looked at W_β through a column 4·1 c.m. long, and endeavoured to get a similar tint with W_α under the other cylinder. For the upper limit the column was 6·95 c.m. long, and for the lower limit 6·2 c.m. long. The mean is 6·57 c.m. I next altered the length of the column over W_β to 11·75. In the other cylinder for the upper limit the column was 14·9 c.m., and for the lower limit 14·8. The mean is 14·85. Hence we have

$$W_\beta K^{4\cdot 1} = W_\alpha K^{6\cdot 57}$$

$$W_\beta K^{11\cdot 75} = W_\alpha K^{14\cdot 85}$$

By cross multiplication and elimination of W^α, W_β

$$K^{4\cdot 1} K^{14\cdot 85} = K^{6\cdot 57} K^{11\cdot 75}$$

Theory requires the sums of the indices to be equal.

The sum on the right hand is 18·32, and on the left 18·95. The difference is, I think, not greater than what might be due to errors of observation.

"Results of Observations of the Variable Star T Aquilæ," by Joseph **Baxendell**, F.R.A.S.

A notice of the discovery of the variability of this star was communicated to the Section on the 12th of November, 1863, and printed in the third volume of the Society's Proceedings. At that time I had observed only one minimum and one maximum, and was therefore unable to determine, with any near approach to accuracy, the length of the mean period and the mean range of variation of magnitude. I have however since observed eight additional minima and eight maxima thus making a total of nine minima and nine maxima, which from the length of time over which the observations extend afford the means of determining the mean period and the mean epochs of minimum and maximum with considerable accuracy.

OBSERVED MINIMA OF T AQUILÆ.

	Mag.	Days in Interval.	Number of Periods.
1863. Aug. 24	11·3	...	...
1864. June 24	11·2	305	2
1865. Aug. 31	11·5	433	3
1866. June 10	10·8	283	2
Nov. 14	11·0	157	1
1867. Sept. 12	10·8	302	2
1868. June 23	10·7	285	2
1878. Nov. 18	10·8	3800	26
1879. Oct. 2	11·6	318	2

Forming the usual equations and treating by the method of least squares we obtain

Mean Epoch = 1868, November, 16·38.
Mean Period = 146·671 days.

OBSERVED MAXIMA OF T AQUILÆ.

	Mag.	Days in Interval.	Number of Periods.
1863. Oct. 25	8·9	...	...
1864. Aug. 23	8·4	303	2
1865. June 30	10·1	311	2
Nov. 22	9·4	145	1
1866. Sept. 6	8·7	288	2
1867. Nov. 4	9·0	424	3
1868. Aug 15	9·0	285	2
1878. Sept. 15	9·0	3683	25
1879. Nov. 22	9·1	433	3

The equations formed from these data, treated by the method of least squares, give:

Mean Epoch=1869, January 23·89.
Mean Period=146·527 days.

The mean of the two values of the period is 146·599 days, and using this to calculate the times of minima and maxima, we have the following differences between the calculated and observed times of minimum:—

Days.		Days.
+ 5·6	... ...	− 8·4
− 6·2	... ...	− 0·2
+ 0·6	... ...	+ 11·4
+ 10·8	... ...	− 13·4
+ 0.4		

The average difference is 6·3 days, or about $\frac{1}{23}$ of the whole period.

The differences between the calculated and observed times of maxima are

Days.		Days.
+ 12·1	... ...	+ 7·1
+ 2·3	... ...	+ 15·3
− 15·5	... ...	− 2·7
− 13·9	... ...	+ 4·1
− 8.7		

The average difference is 9·1 days, or about $\frac{1}{16}$ of the whole period. The irregularities in the times of maximum are therefore greater than in those of minimum. The distribution of the *plus* and *minus* differences in the two series of results does not seem to indicate that any sensible change has taken place in the length of the period.

Like many other variables, T Aquilæ increases in brightness more rapidly than it diminishes, the mean interval from minimum to maximum being 68·5 days, and from maximum to minimum 78·1 days.

The mean magnitude at minimum is 11·08; the lowest minimum yet observed is 11·6, and the highest 10·7, thus showing a range of 0·9 magnitude. The mean magnitude at maximum is 9·07; the highest yet observed is 8·4, and the lowest 10·1, the range of variation at maximum being therefore 1·7 magnitude, or nearly double that at minimum. The extreme range of variation in the star's brightness is 3·2 magnitudes.

The following paper was read at the Annual General Meeting, April 20th, 1880:—

"On an adaptation of the Lagrangian form of the equations of fluid motion, Part I.," by R. F. GWYTHER, M.A.

The object of the Lagrangian form of equation is to follow the motion of a particular element; and, although the Eulerian forms suit the general purposes of fluid motion best, there are certain cases, as that of vortex motion in a perfect fluid (which may be termed steadily progressive) where the course of an element might be investigated with advantage.

For this purpose I propose investigating the course of a fluid element, defined by means of surfaces moving with the fluid, and expressing the results as far as possible in terms of the parts of the element.

This method leads to a more general integral form than that of Weber, and finally exhibits some of the known properties of fluid motion in a novel manner.

If ϕ be a scalar function of the position of a point in the fluid, its total differential after time δt is $D_t\phi\delta t$ and if $D_t\phi=0$, the property of the point, of which ϕ is the analytical expression, is unchanged during the motion. Now let $\phi=\mu$ be the equation to a surface all points on which enjoy the same property, such a surface will, if $D_t\phi=0$, move with the fluid.

The number of independent surfaces of this kind which can pass through any point, or the number of independent integrals of the equation $D_t\phi=0$ is three, or the dimensions of the space considered. For the Jacobian of a higher number would vanish.

The position of a point in the fluid in motion may be represented in terms of the three independent sets of surfaces $\phi_1 = \mu_1$, $\phi_2 = \mu_2$, $\phi_3 = \mu_3$ where μ_1, μ_2 and μ_3 are parameters.

I have avoided the use of theorems and terms such as those of "circulation," as I think they are apt sometimes to override and hide the more important facts which their discoverers intended them to express, and I have endeavoured to bring the fundamental properties to the surface.

Imagine an element of the fluid separated from the rest of the fluid by the surfaces which we shall denote by

$$\mu_1,\ \mu_2,\ \mu_3,\ \mu_1 + \delta\mu_1,\ \mu_2 + \delta\mu_2, \text{ and } \mu_3 + \delta\mu_3.$$

We will investigate expressions for the parts of this element.

First, $\nabla\phi_1$, $\nabla\phi_2$, $\nabla\phi_3$, represent vectors in the directions of the normals to the three surfaces, such that if h_1, h_2, h_3, stand for their tensors, the thicknesses of the element will be given by $\frac{\delta\mu_1}{h_1}$, $\frac{\delta\mu_2}{h_2}$ and $\frac{\delta\mu_3}{h_3}$ respectively.

The directions of the edges are given by $V\nabla\phi_2\nabla\phi_3 = \alpha$ say, and the similar quantities β and γ, so that

$$V\beta\gamma = -S\nabla\phi_1\nabla\phi_2\nabla\phi_3 . \nabla\phi_1 = H . \nabla\phi_1 \text{ say.}$$

The length of an edge will be given by the thickness ÷ the cosine of the angle between the corresponding normal and the edge. Thus length of the edge α is

$$\frac{\delta\mu_1}{h_1} \cdot \frac{h_1 T\alpha}{S\nabla\phi_1\alpha} = -\frac{\delta\mu_1 T\alpha}{H}$$

and the vector edge is $-\frac{\delta\mu_1}{H}\alpha$. ....(1)

Whence the area of the face ϕ, is $\frac{\delta\mu_2\delta\mu_3}{H_2} TV\beta\gamma = \frac{h_1}{H}\delta\mu_2\delta\mu_3$

and the vector of the faces $\frac{\delta\mu_2\delta\mu_3}{H}\nabla\phi_1$ ....(2)

From either (1) or (2) we may get volume of element

$= \frac{\delta\mu_1\delta\mu_2\delta\mu_3}{H^3} S\alpha\beta\gamma = -\frac{\delta\mu_1\delta\mu_3\delta\mu_3}{H}$ ....(3)

The condition of continuity is therefore that $D_t\rho H^{-1}=0$ where ρ denotes the density, and this condition in an incompressible fluid becomes $D_tH=0$. Generally $\rho=H\phi$ where $D_t\phi=0$.

Before proceeding to investigate any forms for σ, the velocity, we will find some expressions from the action of D_t on the expressions just found.

Remembering that $D_t=\frac{d}{dt}-S\sigma\nabla$ we see that

$$D_t\nabla = \nabla D_t + \Delta(S\sigma\nabla)$$

symbolically, where Δ is the same operator as ∇ but acts on the σ only.

Thus $D_t\nabla\phi = \nabla D_t\phi + \Delta(S\sigma\nabla\phi)$

$$= \nabla D_t\phi + \nabla\phi_1 S\frac{d\sigma}{d\phi_1}\nabla\phi + \nabla\phi_2 S\frac{d\sigma}{d\phi_2}\nabla\phi + \nabla\phi_3 S\frac{d\sigma}{d\phi_3}\nabla\phi \text{....(4)}$$

since $\nabla = \nabla\phi_1\frac{d}{d\phi_1} + \nabla\phi_2\frac{d}{d\phi_2} + \nabla\phi_3\frac{d}{d\phi_3}$

[These other two forms of ∇ will also be used

$-H\nabla = \nabla\phi_1 S\nabla\phi_2\nabla\phi_3\nabla + \nabla\phi_2 S\nabla\phi_3\nabla\phi_1\nabla + \nabla\phi_3 S\nabla\phi_1\nabla\phi_2\nabla,$

or $=\alpha S\nabla\phi_1\nabla + \beta S\nabla\phi_2\nabla + \gamma S\nabla\phi_3\nabla,$

since ∇ is in form a vector.]

Let us first apply these results to $S\nabla\phi_1\nabla\phi_2\nabla\phi_3$ or $-H$

$\therefore -D_tH = SD_t\nabla\phi_1.\nabla\phi_2\nabla\phi_3 + S\nabla\phi_1 D_t\nabla\phi_2.\nabla\phi_3 + S\nabla\phi_1\nabla\phi_2 D_t\nabla\phi_3$

$$= -HS\left(\frac{d\sigma}{d\phi_1}\nabla\phi_1 + \frac{d\sigma}{d\phi_2}\nabla\phi_2 + \frac{d\sigma}{d\phi_3}\nabla\phi_3\right)$$

or $\frac{1}{H}D_tH = S\nabla\sigma = -HD_t\left(\frac{1}{H}\right)$ ....(5)

or is evidently rate of compression per unit of volume.

If again we act similarly on α or $V\nabla\phi_2\nabla\phi_3$

$D_t\alpha = VD_t\nabla\phi_2.\nabla\phi_3 + V\nabla\phi_2 D_t\nabla\phi_3$

$$= S\left(\frac{d\sigma}{d\phi_1}\nabla\phi_2\right)V\nabla\phi_1\nabla\phi_3 + S\left(\frac{d\sigma}{d\phi_2}\nabla\phi_2\right)V\nabla\phi_2\nabla\phi_3$$

$$+ S\left(\frac{d\sigma}{d\phi_2}\nabla\phi_3\right)V\nabla\phi_2\nabla\phi_1 + S\left(\frac{d\sigma}{d\phi_3}\nabla\phi_3\right)V\nabla\phi_2\nabla\phi_3$$

$$= S\nabla\sigma.\alpha - S\frac{d\sigma}{d\phi_1}\nabla\phi_1.\alpha - S\frac{d\sigma}{d\phi_1}\nabla\phi_2.\beta - S\frac{d\sigma}{d\phi_1}\nabla\phi_3.\gamma$$

$$= S\nabla\sigma.\alpha + H\frac{d\sigma}{d\phi_1}$$

$$\therefore \frac{1}{H}D_t\alpha + D_t\frac{1}{H}.\alpha = \frac{d\sigma}{d\phi_1}$$

$$\ldots\ \overline{\mathrm{H}}\quad \overline{d\phi_1}\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

whence we obtain $\nabla\sigma = \nabla\phi_1\frac{d\sigma}{d\phi_1} + \nabla\phi_2\frac{d\sigma}{d\phi_2} + \nabla\phi_3\frac{d\sigma}{d\phi_3}$

$$= \nabla\phi_1 D_t\frac{\alpha}{\mathrm{H}} + \nabla\phi_2 D_t\frac{\beta}{\mathrm{H}} + \nabla\phi_3 D_t\frac{\gamma}{\mathrm{H}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(7)$$

$$\text{Now } \nabla\phi_1\frac{\alpha}{\mathrm{H}} + \nabla\phi_2\frac{\beta}{\mathrm{H}} + \nabla\phi_3\frac{\gamma}{\mathrm{H}} = \frac{\mathrm{S}(\nabla\phi_1\alpha + \nabla\phi_2\beta + \nabla\phi_3\gamma)}{\mathrm{H}}$$

$$+ \frac{\mathrm{V}(\nabla\phi_1\alpha + \nabla\phi_2\beta + \nabla\phi_3\gamma)}{\mathrm{H}}$$

of which the vector part is seen to vanish and the scalar becomes -3. Whence

$$\nabla\phi_1 D_t\frac{\alpha}{\mathrm{H}} + \nabla\phi_2 D_t\frac{\beta}{\mathrm{H}} + \nabla\phi_3 D_t\frac{\gamma}{\mathrm{H}} = -D_t\nabla\phi_1\cdot\frac{\alpha}{\mathrm{H}} - D_t\nabla\phi_2\frac{\beta}{\mathrm{H}} - D_t\nabla\phi_3\frac{\gamma}{\mathrm{H}}$$

and $\mathrm{HV}\nabla\sigma = \mathrm{V}(\alpha D_t\nabla\phi_1 + \beta D_t\nabla\phi_2 + \gamma D_t\nabla\phi_3)$ ....(8)

Of these (7) gives the more convenient form, from which we obtain

$$\mathrm{HV}\nabla\sigma = \mathrm{V}\nabla\phi_1 D_t \mathrm{V}\nabla\phi_2\nabla\phi_3 + \&\text{c.}$$

$$= \mathrm{V}\nabla\phi_1 \mathrm{V} D_t\nabla\phi_2.\nabla\phi_3 + \mathrm{V}\nabla\phi_1\mathrm{V}\nabla\phi_2 D_t\nabla\phi_3 + \&\text{c.}$$

and applying to each of these expressions the formula

$$\mathrm{V}\alpha\mathrm{V}\beta\gamma = \gamma\mathrm{S}\alpha\beta - \beta\mathrm{S}\alpha\gamma,$$

we obtain

$$\phi_3\nabla\{\mathrm{S}\nabla\phi_1 D_t\nabla\phi_3 - \mathrm{S}\nabla\phi_2 D_t\nabla\phi_1\} + \&\text{c.}$$

$$+ D_t\nabla\phi_3\{\mathrm{S}\nabla\phi_1\nabla\phi_2 - \mathrm{S}\nabla\phi_1\nabla\phi_2\} + \&\text{c.}$$

$$= \nabla\phi_1\{\mathrm{S}\nabla\phi_2 D_t\phi_3 - \mathrm{S}\nabla\phi_3 D_t\nabla\phi_2\} + \&\text{c.} + \&\text{c.}$$

Whence $\mathrm{S}\nabla\phi_2 D_t\nabla\phi_3 = \mathrm{S}\nabla\phi_3 D_t\nabla\phi_2$, &c., if the motion is irrotational or expressing

$$\mathrm{HV}\nabla\sigma = l_1\alpha + l_2\beta + l_3\gamma$$

we find by operating with $\mathrm{S}\nabla\phi_1$ on each side—

$$-\mathrm{H}l_1 = \mathrm{S}\nabla\phi_1\nabla\phi_2 D_t\beta + \mathrm{S}\nabla\phi_1\nabla\phi_3 D_t\gamma$$

$$= \mathrm{S}(\gamma D_t\beta - \beta D_t\gamma)$$

Whence—$\mathrm{H}^2\mathrm{V}\nabla\sigma = \mathrm{S}(\gamma D_t\beta - \beta D_t\gamma)\,\alpha + \&\text{c.} + \&\text{c.}$ ....(9)

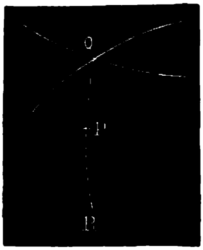
and $\mathrm{V}\nabla\sigma = \mathrm{S}\left\{\frac{\gamma}{\mathrm{H}}D_t\frac{\beta}{\mathrm{H}} - \frac{\beta}{\mathrm{H}}D_t\frac{\gamma}{\mathrm{H}}\right\}\alpha + \&\text{c.}$

and $D_t\mathrm{S}\left(\frac{\gamma}{\mathrm{H}}D_t\frac{\beta}{\mathrm{H}} - \frac{\beta}{\mathrm{H}}D_t\frac{\gamma}{\mathrm{H}}\right) = \mathrm{S}\left\{\frac{\gamma}{\mathrm{H}}D_t^2\frac{\beta}{\mathrm{H}} - \frac{\beta}{\mathrm{H}}D_t^2\frac{\gamma}{\mathrm{H}}\right\}$

$$= \mathrm{S}\left\{\frac{\gamma}{\mathrm{H}}\frac{d}{d\phi_2}D_t\sigma - \frac{\beta}{\mathrm{H}}\frac{d}{d\phi_3}D^t\sigma\right\} = \frac{\mathrm{S}.(\mathrm{V}\nabla\phi_1\nabla)D_t\sigma}{\mathrm{H}} \quad \ldots\ldots\ldots\ldots(10)$$

rion that the vortex motion may exist in the form which it will be seen to take in a perfect fluid. Taking the two other similar quantities, and equating each to zero, we get $V\nabla D_t\sigma = 0$ or $D_t\sigma$ of the form ∇P.

This notation also enables us to give a verbal expression for $V\nabla\sigma$. Thus let PQR lie on a line of intersection of two surfaces ϕ_2 and ϕ_3, and let them be distinguished by ϕ_1, $\phi_1 + \frac{\delta\phi_1}{2}$, and $\phi_1 - \frac{\delta\phi_1}{2}$. Then the vector difference of the velocities at Q and R is $\frac{d\sigma}{d\phi_1}\delta\phi_1$, and resolving parallel to the face ϕ_1 of the element, having P for centre, these differential velocities on the parallel faces ϕ_1, we get



$$\delta\phi_1 V\frac{d\sigma}{d\phi_1}U\nabla\phi_1 \times \text{area} = \frac{\delta\phi_1}{h_1} \times \text{area } V\frac{d\sigma}{d\phi_1}\nabla\phi_1$$
$$= \text{volume} \times V\frac{d\sigma}{d\phi_1}\nabla\phi_1$$

and similarly for the other faces.

$\therefore$ volume × $V\nabla\sigma$ = resultant differential velocity and $V\nabla\sigma$ = mean resultant differential velocity per unit of volume as the result of three shears and rotations, and by a similar method we may find the force due to viscosity, arising in consequence of the motion noticed above, upon the faces ϕ_1, being due to the rate of change of the differential velocities just found. It will plainly be proportional to volume $\times V\frac{d}{d\phi_1}(V\nabla\sigma)\nabla\phi_1$.

II.

The equation of motion is on the generally adopted theory

$$D_t\sigma\nabla + \nabla V + \frac{\mu}{3\rho}\nabla S\nabla\sigma + \frac{\mu}{\rho}\nabla^2\sigma = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

where ∇V represents what would be written in Cartesian,

$$-\int(X\delta x + Y\delta y + Z\delta z) + \int\frac{1}{\rho}(\delta . p.)$$

and therefore assumes each of these quantities perfect differentials, and therefore ρ a function of p only.

We will first take $\mu = 0$.

In any case σ can evidently be written in terms of the normals to the three surfaces ϕ, determining the point at which σ is the velocity. Thus

$$\sigma = K_1 \nabla \phi_1 + K_2 \nabla \phi_2 + K_3 \nabla \phi_3.$$

On the previous assumptions K_1, K_2, and K_3 must be of a definite form, which we proceed to find thus.

$$\begin{aligned} D_t\sigma &= D_tK_1 . \nabla \phi_1 + K_1 D_t \nabla \phi_1 + \&c. \\ &= \left(D_tK_1 + K_1 S\frac{d\sigma}{d\phi_1} \nabla \phi_1 + K_2 S\frac{d\sigma}{d\phi_1} \nabla \phi_2 + K_3 S\frac{d\sigma}{d\phi_1} \nabla \phi_3\right) \nabla \phi_1 \\ &\quad + \&c. + \&c. \\ &= \left(D_tK_1 + S\sigma\frac{d\sigma}{d\phi_1}\right) \nabla \phi_1 + \&c. + \&c. \\ &= D_tK_1 . \nabla \phi_1 + D_tK_2 . \nabla \phi_2 + D_tK_3 . \nabla \phi_3 + \frac{1}{2} \nabla (\sigma)^2 \text{............(2)} \end{aligned}$$

whence the required form of the functions K_1, K_2, K_3 is given by

$$D_tK_1 = \frac{dP^1}{d\phi_1},\ D_tK_2 = \frac{dP^1}{d\phi_2},\ D_tK_3 = \frac{dP^1}{d\phi_3}$$

$$\text{or } K_1 = \frac{dP}{d\phi_1} + \Sigma_1,\ K_2 = \frac{dP}{d\phi_2} + \Sigma_2,\ K_3 = \frac{dP}{d\phi_3} + \Sigma_3 \text{................(3)*}$$

where P is a scalar and $D_t\Sigma = 0$ $\therefore$ Σ a scalar function of $\phi_1\phi_2\phi_3$ only, and σ takes the form—

$$\nabla P + \Sigma_1 \nabla \phi_1 + \Sigma_2 \nabla \phi_2 + \Sigma_3 \nabla \phi_3 \text{.....................(4)}$$

Proceeding to find $V \nabla \sigma$, from this we get

$$\begin{aligned} V \nabla \sigma &= V(\nabla \Sigma_1 . \nabla \phi_1 + \nabla \Sigma_2 . \nabla \phi_2 + \nabla \Sigma_3 . \nabla \phi_3) \\ &= \left(\frac{d\Sigma_3}{d\phi_2} - \frac{d\Sigma_2}{d\phi_3}\right)\alpha + \&c. + \&c. \\ &= \bar{\Sigma}_1\alpha + \bar{\Sigma}_2\beta + \bar{\Sigma}_3\gamma \text{ say.......................(5)} \end{aligned}$$

where $D_t\bar{\Sigma} = 0$.

Whence if the motion is irrotational the Σ's can be included in the ∇P. under the above hypotheses.

Also $V \nabla \sigma$ may be written $H\bar{\Sigma}_1 . \frac{\alpha}{H} + H\bar{\Sigma}_2 . \frac{\beta}{H} + H\bar{\Sigma}_3 . \frac{\gamma}{H}.$, and since $-\frac{\delta\mu_1\alpha}{H}$ &c. denote the edges of the element, the components of

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* Conf. Mr. Hill's paper on some properties of the equations of hydrodynamics, *Q. J. of Maths.*, Feb., 1880.

$V\nabla\sigma$ are $-\frac{H\bar{\Sigma}_1}{\delta\mu_1}$ times the corresponding edge, and if the fluid is incompressible this ratio remains unaltered in time, and the manner of variation, if the fluid is compressible, is indicated by the formula.

Also since $D_t\Sigma = 0$, if $V\nabla\sigma$ is ever $= 0$ it always was and will be $= 0$, and the strength of the section of the vortex on the surface ϕ_1 is $S\frac{\delta\mu_2\delta\mu_3}{H}\nabla\phi_1.\bar{\Sigma}_1\alpha = \delta\mu_2\delta\mu_3\bar{\Sigma}_1$ and is constant in all time.

If we compare the form of σ here obtained with that assumed by Helmholtz, we find that his form is not sufficiently general; since he writes $\Sigma_1\nabla\phi_1 + \Sigma_2\nabla\phi_2 + \Sigma_3\nabla\phi_3 = \nabla\omega$ where ω is a vector with the condition $S\nabla\omega = 0$. That is, he includes two undetermined quantities instead of three, and does not obtain in a distinct form the essential condition, namely $D_t\Sigma = 0$, affecting them.

The quantities Σ are in no way dependent on finite forces which are acting (under the hypothesis), but entirely on the initial conditions (boundary conditions not now being entertained).

Unless the density be a function of the pressure only, the relations just proved will certainly not hold, for then

$$D_t\sigma + \nabla V + \frac{1}{\rho}\nabla p = 0,$$

and $$D_tK_1 + \frac{d}{d\phi_1}\left(V + \frac{\sigma^2}{2}\right) + \frac{1}{\rho}\frac{dp}{d\phi_1} = 0.$$

$$\therefore K_1 = \frac{dP}{d\phi_1} + \Sigma_1 \text{ where } \Sigma_1 = f(\phi_1.\phi_2.\phi_3 \text{ and } t).$$

in which case the fluid pressure will of themselves cause vortex motion.

This seems to be a quite possible condition in a gas rapidly heated or expanded.

Now turning to the terms containing μ and imagining ρ a function of p only, and remembering that μ is independent of ρ, then

$$S\nabla\sigma = \frac{1}{H}D_tH = \frac{1}{\rho}D_t\rho = D_t\log\rho$$

will not necessarily be a function of ρ only, since $D_t\rho$ is not necessarily, nor even generally so. Hence the conclusions we draw above will not generally hold good, for viscous fluids even in the cases where $\nabla^2\sigma = 0$ or $\frac{1}{\rho}\nabla^2\sigma = \nabla Q$, where Q is a scalar. But if the fluid is incompressible in either of these latter cases the theorems hold good.

NOTE.—Since writing the above I have seen a paper by M. Bresse, Fonction des vitesses, extension des théorèmes de Lagrange au cas d'un fluide imparfait (*Comptes Rendus*, March 8, 1880), in which he seeks to show that if $\sigma = \Delta P$ at any time it will remain so throughout the motion. In the investigation, however, the author follows Navier in taking $\frac{\mu}{\rho}$ an absolute constant of the fluid. This, of course, will not lead to correct results for a gas at any rate, and I think his result must be wrong for any liquid even incompressible, as it would follow that vortex motion could not be generated owing to the fluid friction, if for instance it starts from rest.

I should explain the defect in his reasoning thus. Writing ω for $V\nabla\sigma$, and considering ρ constant, the equation affecting ω is

$$D_t\omega + (S\omega\nabla)\sigma + \frac{\mu}{\rho}\nabla^2\omega = 0 \text{ or } V\nabla D_t\sigma + \frac{\mu}{\rho}\nabla^2\omega = 0,$$

from which M. Bresse concludes that if ω is at any time zero it remains so. Now this seems to require that for a small value of ω, $\nabla^2\omega$ should not take a value of an infinitely greater order. It has been shown by Maxwell that if ω be a vector function of any point—$\frac{r^2}{10}\nabla^2\omega$ represents the difference between the value of ω at that point and its mean value over a small sphere of radius r about the point, and therefore I conceive that we can not conclude from the above equation that vortex motion may not arise in lines or surfaces, but merely that it could not appear in a solid form, a form of the existence of which we have no evidence.

The true criterion would be found by equating to zero the expressions found above (10).

Although I conceive that the theorem is not proven for the case which M. Bresse considers where μ "may have any value other than zero," I think that if μ is sufficiently small a proof may be given. For if μ were zero the proposition is true, and the terms owing to which it departs from the truth will appear with μ as a factor, and may therefore be omitted from the term $\mu\nabla^2\omega$; and under this hypothesis if $\mu\nabla^2\omega$ is ever zero it will continue so, and the proposition is completed.

III.

Considering how the portion $\Sigma_1\nabla\phi_1 + \Sigma_2\nabla\phi_2 + \Sigma_3\nabla\phi_3$ of the velocity can be impulsively generated, we see that the initial equation of motion will take the form

$$\sigma = \int_0^T \psi dt + \int_0^T \frac{1}{\rho} p dt.$$

where T is the infinitely small time during which the impulsive force ψ acts and p is the impulsive fluid pressure. Now generally there will be no impulsive forces acting bodily on the fluid, but the velocity will be generated by the impulsive pressures only, and therefore if σ does not satisfy $V\nabla\sigma = 0$, it must be on account of one of two reasons. Either during the impulse ρ does not follow the law of dependence on p, which is highly probable; or p is discontinuous, so that the form ∇p is an improper form.

IV.

The velocity can also be written in a form of simple appearance. Thus—

$$\sigma = -\frac{\dot{\phi}_1}{H}\alpha - \frac{\dot{\phi}_2}{H}\beta - \frac{\dot{\phi}_3}{H}\gamma. \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

since $D_t\phi_1 = \dot{\phi}_1 - (S\sigma\nabla\phi_1) = 0.$

Whence $D_t\sigma = -D_t\dot{\phi}_1 . \frac{\alpha}{H} - D_t\dot{\phi}_2 . \frac{\beta}{H} - D_t\dot{\phi}_3 . \frac{\gamma}{H}$

$$-\left(\dot{\phi}_1\frac{d}{d\phi_1} + \dot{\phi}_2\frac{d}{d\phi_2} + \dot{\phi}_3\frac{d}{d\phi_3}\right)\sigma \text{ by } (6)$$

$$\text{But } (S\sigma\nabla)=\dot\phi_1\frac{d}{d\phi_1}+\dot\phi_2\frac{d}{d\phi_2}+\dot\phi_3\frac{d}{d\phi_3}$$

$$\therefore d\sigma=-D_t\dot\phi_1\frac{\alpha}{H}-D_t\dot\phi_2\frac{\beta}{H}-D_t\dot\phi_3\cdot\frac{\gamma}{H}$$

and the condition for steady motion is $D_t\dot\phi=0$, showing a close resemblance in form between the velocity in this case and the rotation in a perfect fluid.

We may at any time, by elimination of t, find two surfaces of the nature ϕ for which $\dot\phi=0$, and since in steady motion $D_t\dot\phi=0$, the property will continue in the surface. Taking the two such surfaces for ϕ_2 and ϕ_3, they will act as fixed boundaries, their intersections will be the stream lines and the form of σ will be

$$\sigma=-\frac{\dot\phi_1}{H}\alpha \qquad \text{.................................(2)}$$

In spite of the simplicity of this form, it does not appear to yield a convenient form of condition affecting $D_t\sigma$, nor for $V\nabla\sigma$.

The following property can, however, be deduced. In a perfect fluid under conservative forces, we must have $(S\sigma\nabla)$ of the form ∇P

$$\text{But } (S\sigma\nabla)\sigma=\nabla\frac{\sigma^2}{2}+V\sigma V\nabla\sigma.$$

$$\therefore V.\sigma V\nabla\sigma=\nabla Q \text{ say,}$$

$$\text{or} \qquad -\dot\phi_1\overline{\Sigma}_3\nabla\phi_2+\dot\phi_1\overline{\Sigma}_2\nabla\phi_3=\nabla Q$$

whence $\frac{dQ}{d\phi_1}=0$, and $\frac{dQ}{d\phi_2}$ and $\frac{dQ}{d\phi_3}$ become 0 when acted on by D_t, whence Q is a function of ϕ_2 and ϕ_3 only. The existence of this surface Q, on which both stream lines and vortex lines lie, is dependent on the existence of vortex motion, but if the surface exists we may take it in place of the surface ϕ_3 to indicate the stream lines, and then we get $\overline{\Sigma}_3=0$ and $\phi_1\overline{\Sigma}_2=1$, or $\overline{\Sigma}_2=\frac{1}{\dot\phi_1}$, and the rotation would be given by

$$V\nabla\sigma=\overline{\Sigma}_1.V\nabla\phi_2\nabla Q+\frac{1}{\dot\phi_1}V\nabla Q\nabla\phi_1 \qquad \text{....... ....(3)}$$

The cases of steady vortex motion will, I think, generally occur in cases allied to the surfaces of discontinuity investigated by Helmholtz, and the surface Q will then be such a surface.

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